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CONVULSIVE BEHAVIOR IN THE RAT

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I. Introduction

Convulsive behavior, whether in the human or the infrahuman subject, invariably excites the interest of the observer because of its dramatic deviation from typical response patterns. To contribute significantly to our scientific understanding of behavior, however, the observations of seizures under a variety of conditions must be related systematically to one another, with an attempt made to delineate a limited number of causal sequences common to the several situations. So far as I have been able to ascertain, this has not been done for the laboratory rat. This review is designed to bring together some of the salient features of experiments utilizing this animal, with the hope that the juxtaposition of certain findings will stimulate integrative thought in the form of theories of behavior mechanisms.[1]

The independent variable which the majority of investigators have employed to induce seizures in the rat is intense sound. The earlier studies of this type have already been summarized (40). But other experimenters have reported that convulsions can also be produced by such factors as conflict, electrical stimulation, injection with certain drugs, and deprivation of any of several dietary elements.[2] At first glance these major variables may appear so diverse that their logical organization under a single heading is impossible. But further consideration suggests that the interrelationships among the numerous parameters are so complex that it would be almost as artificial to establish five mutually exclusive categories of seizure. The convulsions observed in the several situations may or may not reduce ultimately to

[1] Convulsive behavior has also been studied experimentally in such mammals as the cat (e.g., 43, 116, 138, 140, 173), the dog (18, 23, 70, 147), the mouse (60, 67, 117), the rabbit (139), and the pig (18).

[2] There is no reason to limit the list of experimental convulsants to these few. Cobb (20) summarizes 56 clinical causes and 13 physiologic mechanisms that may underlie convulsive behavior. (Cf. also 172.)

a common physiological denominator. But even if there still remain two or more "kinds" of convulsion, an attempted unification at the present stage may well lead to the development of new methods of studying the nature of one or another of these varieties.

The question may legitimately be raised as to the psychological importance of investigations of convulsions which adopt the rat as subject. Enough has been written in defense of the comparative method that little need be added here. It should be stressed, however, that no transfer of principles from rat to human behavior can safely be made until a clearer insight into the complex problem of the rat behavior itself is achieved. For the present, then, this field remains of direct interest primarily to the animal experimenter.

There are three principal reasons why the investigation of induced convulsions is of value.

1. The appearance of the seizure may be influenced by a number of factors—drug injections, dietary deficiencies, the physiological concomitants of aging, chronic emotionality, and conflict, for example. As a consequence, this pattern of behavior might be employed as an assay indicator of the presence and strength of such factors. With few exceptions (e.g., **8**), this possibility has been little exploited, probably because of insufficient standardization of the indicator itself. Experiments with this orientation are presumably to be based on such relationships as are discussed in Section IV of this review.

2. The convulsion may alter other behavioral and physiological characteristics of the organism. Several investigators have reported the modification of learned patterns following seizure, and others have attempted to demonstrate changes in diurnal activity, physiological condition, and general reactivity. These experiments have for the most part failed to yield unequivocal results, but continued work in this direction will do much to clarify the neurological bases of these behavior segments and contribute to our fundamental understanding of such matters as the learning process. Section V summarizes the few studies bearing on this type of problem.

3. The nature of the convulsion is of considerable interest for its own sake. Its outward resemblance to human epileptic and induced convulsions has frequently been pointed out, and the similarity of appearance may be paralleled by a similarity of mechanism. If it should eventuate that nothing can be contributed to the understanding of these human syndromes, at least there is every reason to expect that the controlled observation of the rat seizure will expand our knowledge of basic neurophysiological processes. Theories attempting to describe the physiological substrate are a step in this direction; they are considered in Section VI.

II. The Classification Of Convulsions

From the historical point of view, one might be justified in setting up five categories of convulsions as observed in the rat, each "kind" corresponding to a relatively unique set of experimental conditions.

That this is probably too simple a treatment has already been noted, will be elaborated upon in Section IV, and will be the premise upon which the logical organization of Sections IV and VI will depend. In the present section the oversimplified five-fold classification will be outlined, each classification based upon one major causal factor or kind of factor ("invariable antecedent") that has been proposed by some investigators as accounting for certain convulsions.

Conflict-induced Convulsion ("Experimental Neurosis")

The experiments which gave the initial impetus to the psychologist's interest in rat seizure behavior were designed to demonstrate the efficacy of conflict in the production of behavior aberrations. In 1938 Maier and Glaser (**98**) reported that rats forced by an air blast to respond to an insoluble problem in a Lashley jumping apparatus sometimes exhibited behavior termed by them "experimental neurosis." The rat might be confronted, for example, by just one card, responses to which had in the past been punished. The resultant neurotic behavior was described (**92**) as including circular running at terrific speed, during which collision with objects in the room was frequent; tonic and clonic spasms of variable extent and intensity; and a peculiar hopping locomotion. Following this active phase a passive phase occurred, during which the apparently unconscious rat would maintain unusual postures imposed upon it and was extremely insensitive to stimulation. Recovery was gradual, and within a few minutes the rat appeared quite normal, although his cage behavior was described as typically "retiring" (**92**, p. 30).

In later researches Maier and his collaborators found that the "neurotic" reactions were inhibited by preliminary stimulation with sound, lowering of body temperature, excessive muscular activity (**94**, **101**), the development of "abortive" jumping (**93**) or position habits (**103**), and the previous occurrence of attacks (**99**). On the other hand, subconvulsive doses of metrazol (**106**), increased intensity of the auditory stimulus (**99**), and confinement in a small, opaque stimulation chamber (**99**, **101**) tended to increase the convulsive response to the "conflict" engendered by the noisy air blast. Hereditary influences were also demonstrated (**95**, **100**).

The first doubt that this abnormal pattern was dependent upon conflict was raised by the report by Morgan and Morgan (**122**) that rats lacking discrimination training and stimulated in their home cage by the high-pitched tones of an air blast sometimes exhibited seizure behavior outwardly identical to that of Maier's rats. Indeed, as was

later pointed out (**118**), every instance of the "neurotic" seizure reported by Maier occurred in the presence of loud noise, and some were observed completely outside the discrimination situation, when the only apparently invariable stimulus was intense sound. Other stimuli, electrical and mechanical, were adopted by Maier to force the rats to jump in the difficult problem situation, but none led to the convulsive pattern. The suggestion was inevitable that it was sound, rather than discrimination-conflict, that precipitated the convulsion. Other investigators (**4, 38, 73, 86, 109, 126, 130, 148, 151, 154, 155**) have corroborated the observation that untrained animals can often be made to convulse by a few seconds[3] of auditory stimulation. In fact much of Maier's later work has been concerned with the production of seizures by various sound sources, with no attempt made to induce conflict by discriminative or other training (cf., e.g., **94, 95, 99, 100, 101, 102**). Because sound seems to be the most invariable antecedent of the convulsive seizure, because no conflict between formally learned tendencies is required, and because the changes originally described by Pavlov (**137**) as characterizing experimental neurosis (temporary loss of discrimination ability, resistance to the experimenter, and alterations in general modes of behavior outside the experimental situation) have not been demonstrated, the majority of investigators now prefer to regard the behavior as something other than conflict-induced experimental neurosis (e.g., **10, 41, 83, 121, 153**).

Maier's original position is, however, not undefended. Maier himself states that although ". . . auditory stimuli are basic in the production of abnormal behavior" (**99**, p. 29), "The experimental neurosis . . . is not a response to air alone" (**92**, p. 67). The situation involving simple auditory stimulation can be regarded as conflictful, "since the auditory stimulus . . . [gives] rise to no specific response but [is] excitatory in nature and the enclosed space restrict[s] general behavior and serve[s] as the inhibitory aspect of the situation" (**93**, p. 369). Bitterman (**12, 13**) has argued in the same vein, holding that in the stimulation situation there is conflict between the excitatory auditory stimulation and the inhibitory physical barriers (including empty

[3] Smith (**153**) found that certain peculiar "premonitory" behavior was followed by the full seizure in more than 80% of his 1309 rats. This frequent coincidence would make plausible the inclusion of the premonitory behavior as the first part of the convulsive pattern. Using this as the indicator of onset, many attacks were observed to begin within one second after the beginning of the auditory stimulation. The more obviously convulsive components were observed to have latencies of 1–7 sec. in 50% of his cases (cf. **13**, p. 118).

spaces). More direct support of the conflict (vs. auditory) hypothesis is found in the results of two experiments in which sound was apparently not a factor. Griffiths (**56**) describes in considerable detail convulsions occurring in a rat forced by electric shock to respond in a difficult choice situation, and Bitterman (**14**) reports one seizure in a learning situation in which shock was the punishing agent. If additional experimental evidence of this nature is forthcoming, and if it cannot be attributed to such an alternative factor as dietary deficiency or electrocortical shock, the existence of the "conflict-induced convulsion" category will become more logically acceptable.

Sound-induced Convulsion ("Audiogenic Seizure")

Although the physiological explanation is still obscure, there is no question but that exposure to intense auditory stimulation will cause a large number of rats to exhibit a pattern of response deviating markedly from typical laboratory behavior (**4, 38, 73, 86, 99, 109, 126, 130, 148, 151, 154, 155**).[4] Sound sources successfully used include an air jet, the Galton whistle, buzzers and bells, jingling keys, and an oscillator. No previous training is necessary, nor has any extrinsic factor but sound been demonstrated to be essential. With a latency varying from less than five seconds after the onset of stimulation to somewhat more than one minute, the previously "normal" rat begins a reaction closely reminiscent of the pattern described by Maier as experimental neurosis. The pattern is far from stereotyped, various combinations of wild running, tonic-clonic convulsions, and stupor being possible.

This reaction was probably first mentioned in 1924 by Donaldson (**28**, p. 134), who, in describing the acute hearing of the white rat, noted the "almost maniacal running and jumping" that often occurred when a bundle of keys was jingled before the home cage. Investigators of the effect of magnesium deficiency in the rat described as early as 1932 a convulsive sequence that occurred "spontaneously" or upon auditory stimulation (**51, 85**), and Dice (**27**) reported a similar pattern in "epileptic" mice exposed to the sound of jingling keys. It was not until 1939, however, that this atypical response came to be widely known to comparative psychologists, largely as the result of the controversy over the nature of the Maier-type "experimental neurosis." Because of the apparent primacy of the auditory component in inducing the convulsive reaction, Morgan and Waldman (**123**) suggested the descriptive phrase

[4] Sound has also been observed at times to induce various "abnormal" patterns in the human (e.g., "musicogenic epilepsy," **23**), the dog (**23, 124**), the cat (**5**), the guinea pig (**171**), and the mouse (**27, 117, 168**).

"audiogenic seizure," which has become the most widely accepted term.[5]

The audiogenic seizure is influenced in both its pattern and frequency of occurrence by a number of secondary factors, so that susceptibility figures within a colony and in a particular testing situation range from 10% or less (**86, 122**) to well over 90% (**34**). The inhibiting effect of the following factors has been demonstrated: strenuous activity before or during the stimulation period (**2, 76, 101**); extreme restriction of movement (**54, 88**); the establishment of position habits (**103**) or the development of abortive jumping (**93**) when the stimulation is in the Lashley apparatus; the appearance of a fear reaction (**2**); previous exposure to the convulsion-producing sound (**11, 38, 76, 99, 153**); electrification of the floor during the auditory stimulation (**15**); the provision of a "psychological shelter" (**56**); taming (**76**); temperature changes (**101**); chronic "emotionality" (**11, 114**); vagotomy (**88**); partial curarization (**88**); and the administration of such substances as adrenin (**2, 87**), dilantin sodium (**21, 54, 148**), adrenal cortex (**54**), thiamin hydrochloride (**59, 81**), atropine (**72, 73**), B-complex vitamins and minerals (**131, 133, 134**). The frequency or severity of seizure has been found to be increased by such means as repeated anger stimulation (**163, 164**); increased intensity of auditory stimulation (**48, 120, 121**); chronic disturbances induced in other situations (**75, 76**); increased conflict, e.g., as provided by additional barriers to escape (**99, 103**); cutaneous stimulation furnished by water spray (**97**); temporary deprivation of food and drink (**38**); lactation (**127**); perhaps prenatal treatment with sodium bromide (**64**); B-complex avitaminosis (**133**); magnesium deficiency (**135**); and injection of subconvulsive doses of strychnine (**2, 153, 154**), metrazol (**78, 103, 106, 145, 154**), caffeine sodium benzoate (**154**), sodium phosphate (**50**), and coramine (**166**). Age differences (**31, 33, 39, 102**), sex differences (**33**), genetic background (**34, 39, 57, 58, 95, 100, 113**), blood composition (**86**), inanition (**132**), cortical insult (**7, 153, 169**), punishment for seizure (**107**), and eserine (**72, 73**) and phenobarbital (**77**) injections have also been studied with respect to their effect upon audiogenic seizures.

The nature and significance of the sound-induced seizure have been considered variously. The majority of investigators seem to be seeking an explanatory principle involving a rather direct overloading of the nervous system by the auditory stimulation. A few (**12, 93**) still hold

[5] Other terms applied to the behavior in question include "audio-epileptic seizure" (**153**), "epileptoid convulsive reaction" (**4**), "noise-induced seizure" (**72**), "noise-fright convulsion" (**155**), and "sonogenic convulsion" (**117**).

the seizure to be a manifestation of strong conflict between opposed tendencies, and others (**2**, **56**, **160**, **164**) emphasize the intense emotional components of the precipitating situation. It may be, as Patton has intimated (**129**), that the so-called audiogenic seizure is simply a mild form of the disorders which clearly result from severe nutritional deficits. This would appear to be an experimentally fruitful suggestion, the verification of which will probably wait on more refined neurophysiological and biochemical techniques. In any case, the apparently intimate connection with convulsions otherwise produced makes it relatively meaningless at this juncture to consider the audiogenic seizure as an isolated phenomenon.

Electrically-induced Convulsion

Another method of producing convulsive behavior in the rat is the passage of electric current through the skull (Ferrier, 1873, **172**). The technique is analogous to human electroshock therapy, and the results are in part similar. The electrodes are variously applied: to the shaven skull (**80**), one to the head and the other to the tail (**125**), to the ears (**29**, **149**, **155**), to either side of the head between the eyes and the ears (**48**), or on a line one centimeter behind the eyes (**162**). The shocks employed to produce the generalized convulsion have included 10-20 volts, 30-50 milliamperes, 0.3 seconds (**66**); 40 v., 20-25 ma., 2 sec. (**80**); 70-85 v., 0.2-0.6 sec. (**29**, **149**, **155**, **162**); 120-145 v., 50-100 ma., 0.1 sec. (**174**); 600 v., 4.5 ma., 2 sec. (**125**); and 700 v., 9-14 ma., 0.75-0.95 sec. (**48**). At these intensities a convulsion consisting of tonic flexion, often succeeded by extension and clonic contractions, occurs in most subjects, usually followed in 10-30 sec. by a period of hyposensitivity and reflex depression. This reaction is attributed by Kessler and Gellhorn (**79**) to a hyperglycemic condition, in which the autonomic balance is on the sympathetico-adrenal side. Wortis *et al.* (**174**) suggest that the mechanism involves inhibition of the oxygen uptake of the brain (cf. **25**).

Heilbrunn (**66**) has found that light ether anesthetization attenuates or abolishes completely the electroshock convulsion, and according to Page (**125**) alcohol tends to raise the shock threshold. On the other hand, atropine sulphate, synthetic vitamin K, calcium gluconate, thromboplastic suspension of brain substance, and combinations of these (**66**), and adrenalin (**125**) have been found ineffective in diminishing the effect of the shock.

When a shock of decreased intensity or duration is substituted, the generalized convulsive pattern is sometimes replaced by one resembling

the audiogenic seizure more closely, in that the tonic-clonic phase is preceded by a period of running. This running has been described by Page (**125**) as being more controlled and "less forced" than that typical of the audiogenic pattern. However, Golub and Morgan (**48**), Stainbrook (**155**), and Siegel and Lacey (**150**) seem to have succeeded in reproducing more exactly the running picture characteristic of the audiogenic convulsion. This production of the pattern attributed by some to conflict or to sound is presented as strong evidence that neither of these factors is as significant in the etiology of the running-convulsive behavior as sometimes implied. Such reasoning is not indisputable but at least suggests that a rapprochement of the differing viewpoints and experimental findings, perhaps at the neurophysiological level, may be mutually enlightening.

Drug-induced Convulsion

As early as 1864 convulsions were produced in experimental animals by the injection of foreign substances (Morcé, **172**). With the widespread adoption of drug therapies, it was to be expected that a large number of investigators should turn to the rat in an effort to ascertain the nature and therapeutic mechanism of the convulsions. Metrazol has been the most frequent convulsant studied (e.g., **68**, **80**, **89**, **142**). In one investigation (**105**), a 1% solution of metrazol in 0.85% saline was injected intraperitoneally, with an average convulsive threshold found in one group to be about 40 mg. per kilogram body weight, and the latency of attack approximately three minutes. Tonic flexion, usually with torsion of the spine, is the most characteristic feature of the convulsion, usually followed by a clonic stage. A passive phase terminates the seizure, sometimes preceded by a brief period of rigidity or stiff hopping. Recovery appears complete within a few minutes. The tolerance to the drug has been found to change with repeated administrations, increasing when treatment is given on alternate days, decreasing when injection is weekly (**143**). The mechanism underlying the seizure has been suggested to involve increased cerebral metabolism and anoxia (**25**, **174**), and increased autonomic (especially sympathetico-adrenal) excitation (**37**). The discovery that rats maximally susceptible to sound-induced convulsions have on the average a lower drug threshold and a metrazol convulsion of longer latency and duration than do rats less susceptible to sound-induced seizures (**105**, **144**) underlines the necessity of interrelating the patterns appearing in these different experimental situations.

Insulin has also been employed as a convulsant. Berman and Riess

(**9, 141**) injected one unit per 12 grams body weight, which led in approximately 50% of instances to severe convulsion. Hypoglycemia is very probably a causal factor (**8**), and inhibition of brain oxygen uptake is also probable (**174**).

There is no reason to limit the list of convulsants administered to the rat to these two, as any list of pharmacological properties will testify (**69**). The fact that subconvulsive doses of many chemicals modify the convulsive response to sound or conflict enhances the likelihood that common physiological bases for the different "kinds" of convulsion may be discovered through this type of investigation.

Convulsion Associated with Dietary Deficiency

Rats deprived of one or more elements of the vitamin B complex (**17, 131, 132, 134**), normal dietary magnesium (**26, 124, 135**), or calcium and vitamin D (**50**) become abnormally sensitive to stimulation. If the deprivation is begun early enough these rats will respond to mild auditory or cutaneous stimulation with behavior described as "epileptic" (**19**) or "uncontrolled" (**84**)—running fits accompanied or followed by convulsions (**24**). Concomitant physiological changes are often apparent; Kruse *et al.* (**85**) found that hyperemia and vasodilatation began about four days after introduction of a magnesium-low diet and subsided 12 days later, with the onset of seizures, death usually intervening within another four weeks. As the deficiency increases, convulsions are often observed to occur spontaneously, i.e., without obvious relation to particular environmental stimuli (**52, 135, 136**).

The behavioral similarity of the attacks occurring under conditions of deprivation to the "conflict-induced" convulsion and the "audiogenic" seizure might inspire a search for a common physiological etiology. This approach is still more strongly suggested by the sequence of events as deprivation becomes progressively more severe. For example, with a diet including 1-2 mg. of magnesium per 100 g. of dry food, convulsive seizures occur only upon application of a stimulus such as an air blast (**51, 52**). With only 0.4 mg. of magnesium per 100 g. of dry food, convulsions occur in response to less and less intense sounds as the period of deprivation continues (**53, 85**), and finally "spontaneous" seizures may be observed (**135**). Pyridoxine deficiency runs a similar course (**136**). (It is pertinent to note that sounds are by far the most effective stimuli for releasing these convulsive seizures.) One tentative explanation of the difference between various strains of rats in susceptibility to sound has been a difference of diet, which becomes an obvious possibility in light of the above studies. Patton (**129, 136**) by implication

goes further. With a mildly deficient diet he obtains approximately 100% convulsive responses to sound, and with more severe deficiency "spontaneous" seizures in nearly all rats; with a diet fortified by mineral and vitamin supplements the frequency of "audiogenic seizure" is close to zero. It is no great extension of this relation to propose that underlying all sound-induced convulsions is a dietary deficiency, and that the Maier-type "neurosis" is no more conflict-induced than were the symptoms attributed by Kruse *et al.* (**85**) in 1932 to magnesium deficiency. Until these possibilities are eliminated, it will be well to consider the general topic of "convulsions" as a whole, rather than by categories, evaluating each of the many factors that may modify the behavior (cf. Section IV).

III. The Convulsive Patterns

Before a decision can be reached as to the unity or diversity of the "types" of rat convulsions, it is obviously necessary to compare the patterns appearing in the several situations. It must be confessed that no very confident comparison can be made at present since because of the inherent variability of the patterns, or deviations reflecting testing differences, or inaccuracies of the observations, there is imperfect agreement as to the precise form of behavior that characterizes each "type." Whether or not differences between the categories are reliably greater than differences within a single category, outweighing the similarities, probably will be settled only on the basis of refined photographic techniques.

Convulsion Associated with Auditory Stimulation. Whether the more significant causal factor is sound or conflict, it is agreed that "neurotic" or "emotional" convulsions in the absence of intense sound stimulation have been described in only one rat (**56**), and this single description offers little detail. Further, the proponents of the neurosis hypothesis seem to impute "conflict" to any situation which includes inescapable stimulation of high intensity (e.g., **13**, **93**). It is therefore logical to combine under one heading the descriptions of the seizures attributed by their observers to either variable.

Just how complete a pattern of behavior should be included as audiogenic or neurotic convulsion is open to question. Maier (**92**) and the Morgans (**122**) originally incorporated the early "blind" running into their description, probably because of its unusual nature and because no true convulsion ever appeared without a preceding running phase. Most writers have followed this practice, although the question immediately arises as to the meaning of the typical running pattern when it occurs alone or, for that matter, of the so-called premonitory behavior

or the substitute activity that so frequently precedes or takes the place of running. The criterion of attack will obviously require careful definition before a truly satisfactory conception of the problem can evolve; numerous controversial issues have remained unsettled largely due to an unrecognized disagreement as to terminology.

Accepting the most complete sequence as constituting the convulsive seizure, we find almost innumerable combinations of the premonitory, running, tonic, clonic, and passive phases. (For detailed descriptions of variations, cf. **7**, **92**, **99**, **119**, **153**, **157**.) Classifications have been suggested for certain combinations of symptoms (**99**, **157**), but the reality probably approaches a continuum (or several continua) of severity and duration. Any generalized description, such as follows, is thus misleadingly simple, although possessing the merit of facilitating comparison with the convulsions evoked by electricity, drugs, and dietary deficit.

After the initial startle response to the onset of the stimulus, any of a number of patterns may appear. Some animals appear to ignore the sound source, others seek to retreat from it, a few actively attack it. Many rats crouch motionless or try to burrow beneath the floor covering or their fellow subjects. In most nonseizure trials some sort of "substitute behavior" appears—rapid nose or ear rubbing, teeth chattering, chewing, shivering, shaking, vibrissae twitching, body cleaning, or restless head and body movements (**2**, **88**). In trials culminating in a seizure, behavior described by Smith (**153**) as a "motor aura" usually occurs, typically consisting of brief quick runs, jerky sidling or backward steps, or pivoting movements of the head and body (cf. also **63**, **114**, **151**). After a latency ranging between one second and one minute (**4**, **7**, **119**, **153**), the active phase begins with a running period, sometimes of gradually accelerating speed, sometimes of a very explosive nature, and in any instance finally attaining an unusually high velocity. The running may or may not follow a circular path, but almost always seems undirected with respect to obstacles such as furniture and other rats. This phase may be interrupted by one or more quiet periods, may terminate suddenly in what resembles coma, or may continue with diminishing vigor until exhaustion. Most characteristically, it leads directly (after an average of 15–20 sec.) into the convulsion proper. This may be predominantly of tonic extension, reminiscent of decerebrate rigidity (**99**, **114**), of incoordinated clonic beating of the limbs, or may combine both tonic and clonic aspects. During the tonic-clonic phase (average total duration 30–40 sec.) the following are sometimes observed: vocalization, exophthalmus, excessive salivation, cyanosis, ejaculation, diminished respiration, incontinence, dilated pupils, piloerection, retraction of testes, relaxation of the vaginal opening, and peripheral vasodilatation, with death intervening in a very few cases. The rat may continue to exhibit clonic jerks (e.g., spasmodic jumping) with decreasing frequency as long as stimulation continues, or may lapse into the

phase usually described as comatose. Most animals are very unresponsive to stimulation during this passive phase, with reflexes depressed to varying degrees. Spontaneous activity is virtually abolished, and the animal is nonresistant to handling. This condition may last for longer than 15 minutes, although gradual recovery of spontaneous activity usually begins within two or three minutes. (The decreased activity is probably not simply the result of fatigue, for as Beach and Weaver (**7**) point out, the duration of the coma is often inversely related to the amount of exertion occurring in the active phase.) In a few instances hypersensitivity or renewed clonic activity succeeds or takes the place of the typical stuporous condition (**63**, **117**, **153**). A recovering animal appears extremely fatigued, and quantitative recording reveals that the frequency of movement is subnormal for several hours (**42**). For some hours following a seizure the threshold to induced convulsion is markedly elevated.

Accompanying this pattern of overt behavior, a number of internal changes have been detected. Lindsley, Finger and Henry (**88**) demonstrated alterations in the electrocortical activity paralleling the external manifestations of the attack, and although technical difficulties obscured detailed analysis, many features in common with the EEG in human convulsive seizures could be recognized. The heart rate also reflects the progress of the seizure, fluctuating markedly just before the attack, reaching a high level during the active phases of the convulsion, and dropping far below normal and showing various irregularities during the comatose stage (**47**, **76**, **88**, **95**, **109**, **111**, **112**). The passive phase is characterized also by acidosis, decreased blood CO_2, and decreased clotting time of the blood (**22**).

The only attempt to distinguish the pattern occurring in response to sound alone from that resulting from auditory stimulation in a formal conflict situation is that of Maier and Glaser (**99**). They suggest that in the former situation the extension component of the convulsion is relatively more intense and the passive phase less pronounced. However, these differences could easily be attributed to uncontrolled variables, especially age, differentiating the two groups compared, and Maier and Glaser emphasize that "the similarities are too great to consider these differences as symptoms which distinguish between different abnormalities" (**99**, p. 12).

Electrically-induced Convulsion. There are at least two different forms of seizure resulting from electrical stimulation of the brain, depending on the intensity of the current. At higher intensities, the onset of the current is marked by a "start," with the spine flexed inward, hind legs drawn up, and front legs brought down (**125**). For the duration of the shock (conventionally 2 sec. or less) the animal remains rigid. He may then pass into a state of tonic rigidity, with the back arched, the tail stiff, the legs sometimes briefly flexed (**79**) and then extended

as far down as possible, and the eyes tightly closed. Gradually the fine tremors of this stage give way to clonic movements (involving principally the hind legs, **48**, **155**) of decreasing frequency and increasing amplitude, with frequent urination, defecation, and sometimes ejaculation. After about 30 sec. coma supervenes. This varies in character, but usually begins with a very deep phase during which the rat is nonresistant and the reflexes abolished even more completely than in the sound-induced seizure. After a few minutes there often succeeds a hyperexcitable stage during which "blind rage" reactions can be observed.[6] As measured by maze activity, the recovery period is longer than in the sound-induced seizure (**160**). As soon as the animal begins to be active, a second convulsion can be elicited, in contrast to the longer refractory phase following the audiogenic seizure.

With less intense shock, the tonic phase may be omitted, with the clonic and comatose phases following directly the primary jerk (**48**). The more characteristic pattern, and of particular interest because of its similarity to the sound-induced seizure, includes a vigorous running phase after the initial "start," which may alternate with or be followed by tonic rigidity and/or clonic spasms before the coma begins. Golub and Morgan (**48**), Siegel and Lacey (**150**), and Stainbrook (**155**, **157**) describe this seizure as resembling the audiogenic convulsion closely, although the latter observer notes a greater resistance to handling and more promptly returning activity in the postseizure state. On the other hand, Page (**125**) believes that the running that is induced electrically appears less "forced" than that following auditory stimulation.

Physiological changes accompanying the electroshock convulsion include a relative cerebral anoxia, attributed to increased cerebral metabolism (**25**), and extensive hemorrhages in the pia-arachnoid and in the parenchyma (**66**).

Drug-induced Convulsion. There are a great many drugs which, injected with the proper dosage, produce convulsions in the rat (**69**). Because of the interest in the insulin and metrazol therapies for human behavior disorders, most of the comparative psychologist's attention has been directed toward these convulsants. For the most part, however, the emphasis has been more upon the effects of these drugs on learning and retention than upon the convulsive patterns themselves.

Berman and Riess (**9**) injected 1 unit of insulin per 12 g. body weight. After about 40 minutes, lethargy and/or coma were observed. In nearly 50% of the subjects severe convulsions were also observed, involving especially the head region. Recovery appeared complete at the end of 8 hours.

Maier and Sacks (**105**, **144**) give a more detailed analysis of the

[6] Cf. the "sham rage" reactions of the decorticate dog and cat (**5**). It has been suggested that the convulsive seizure may sometimes bring about a sort of temporary "functional decortication."

metrazol reaction. The tonic spasm is the most characteristic sign of the syndrome, but two different patterns distinguish rats minimally and maximally susceptible to sound-induced seizures. In the first group, intraperitoneal injections are followed in 2–4 min. by a few twitches of the head and a sudden tonic flexor spasm involving the head and all four extremities, usually with spinal torsion. This is replaced by clonic movements of the forepaws, with the combined tonic-clonic period lasting perhaps one minute. The rat then becomes relaxed and passive to handling (slow pulse, **47**), with recovery apparently complete within a few minutes. In the group that responds convulsively to sound, the onset of metrazol convulsion is frequently delayed for several minutes. The tonic flexor spasm and forepaw clonus are usually present, but these stages are followed by a period of rigidity, during which the rat is extremely hypersensitive to stimulation. There then usually occurs a period of rigid hopping (as seen in the "neurotic" rats described by Maier, **92**), sometimes leading to a second period of rigidity and repeated convulsions. In both groups of animals it appears that repeated drug injections or, to a lesser extent, sound-induced convulsions, lead to an elaboration of the pattern as well as a decreased threshold of convulsive dosage.

Convulsion Accompanying Dietary Deficiency. Most of the studies describing convulsions in nutritionally deficient animals have utilized diets low in magnesium (**53**, **85**, **124**, **136**) or certain elements of the B-vitamin complex (**17**, **24**, **84**, **134**). The two patterns, whether precipitated by auditory stimulation or occurring "spontaneously," seem too similar to allow any clear differentiation. The patterns both typically begin with high speed racing fits, often describing a circular path, and frequently punctuated by vocalization. The rat may then fall on his side for a brief period, rigid and with breathing suspended. Tonic spasms and clonic twitchings then usually occur. Kruse, Orent and McCollum (**85**) report no incontinence in magnesium tetany; Chick, El Sadr and Worden (**17**) observed both urination and vomiting during convulsions in rats deprived of B_6, which coincides more closely with the descriptions of sound-induced convulsions. Sometimes only the running aspect is observed (**17**) and sometimes multiple seizures take place (**65**, **85**). Usually a coma follows the active phase, marked by greatly depressed reflexes and, as in audiogenic convulsions, a slow and irregular pulse (**17**, **53**). The total pattern, active and passive, usually lasts 2–4 minutes, with the gradual recovery coming to the head region first. Death terminates the seizure more frequently than is typical of the other convulsive states.

As Patton, Karn and Longenecker (**134**) point out, these seizures can be remarkably similar in outward appearance to sound-induced convulsions. To what extent the similarity extends to the physiological level has yet to be determined.

On the basis of the best descriptions at hand, it would appear that the five "types" of convulsion have much in common, as far as outward manifestations are concerned. There is no reason to distinguish between the convulsions precipitated by sound and those (if any) based on sound-induced conflict. While the pattern of response to high intensity electric current lacks the running phase characteristic of sound- or conflict-induced seizure, lower intensities do produce the running-tonic-clonic-coma sequence. Although it is not clear that these sequences are identical, at least similarities are striking enough to inspire comparison at the neurophysiological level. Similarly, the metrazol shock includes components suggestively reminiscent of the audiogenic seizure, although lacking the running phase. Finally, the descriptions of audiogenic and electrogenic seizures are virtually duplicated by those of attacks occurring at certain stages of dietary deficiency. There is little evidence in the overt picture to require a differentiation of this nutritional syndrome from the seizures produced by auditory or electrical stimulation in the presumably well-fed rat.

The external resemblances do not of course establish the community of physiological or psychological mechanisms. The results of more precise investigation may invest apparently slight divergencies with increased significance. Until then, however, we may well orient our experiments around complex patterns of variables which influence convulsions in general, rather than around the more limited number of etiologically designated categories.

IV. Variables Influencing Convulsive Behavior

The resemblance of the convulsive symptoms induced under the several sets of circumstances is not the only hint that a consideration of convulsive-behavior-in-general may be fruitful. Several investigations make very clear that the appearance of one "kind" of convulsion in a given rat may be in part a function of experience with another "kind" of convulsion, that what appears to be a major factor in one "kind" is a contributing factor in a second "kind" and vice versa—in short, that the interrelationships among the numerous variables are so complex that the intercategory lines are crossed too often to allow their strictly divisional meanings to be maintained. A few examples will clarify the point.

1. The amount of metrazol required to produce convulsion, as well as the precise form of the seizure obtained, is a function of the susceptibility of the rat to sound-induced seizure (**105**). Further, both the threshold and form of metrazol convulsion are altered by the experience of sound-induced seizures (**144**). On the other hand, subconvulsive doses of metrazol increase the likeli-

hood of sound-induced seizure, even though such attack still possesses the unmistakable characteristics of sound-seizures (**78**, **103**, **106**, **145**, **154**).

2. The percentage of a population susceptible to audiogenic seizures varies with diet (**50**, **133**, **135**). While convulsions in nutritionally deficient rats sometimes appear "spontaneously," sound stimulation releases them most readily (**53**, **134**).

3. According to some investigators, the convulsive reaction to sound is increased if conflict based on difficult discrimination is added (**99**) or if the animals have been brought to a condition of chronic emotional disturbance (**75**). Conversely, sound stimulation has been found to be a peculiarly effective means of inducing conflict that may lead to seizure (**99**).

No behavior pattern can be said to be the result of a single causal factor. Every response is the product of an interaction between the complex environment and an organism whose immediate state has been influenced by a multitude of conditions. It is thus a questionable procedure to assign the responsibility for any reaction or type of reaction to one characteristic of the environmental-organismic situation. Nor is it invariably possible so to manipulate the relevant variables that even the major antecedent can be segregated. In the problem under consideration, for example, it seems that according to one personal definition of "conflict" it is virtually impossible ever to provide an intense and continuing stimulus without concurrently inducing conflict (**13**). At the same time, the studies of Patton and his collaborators (**127-136**) make it clear that the dietary background of a rat can never be ignored; it is difficult to know with certainty that the diet of a particular animal is adequate, that with improved nutrition the situation presently leading to convulsion might not lose its effectiveness. Likewise, it is not easy to limit the exteroceptive stimulation impinging on the possibly malnourished subject to a degree that it can be ruled out as a precipitant of seizures. For these reasons, as well as for those already stated, it seems defensible at present to avoid dogmatic etiological classification of convulsions, and simply to outline the results of variation of a number of parameters, hoping that this form of organization may lead to new insights regarding the physiological (or psychological) bases of the behavior.

Auditory Stimulation

With the exception of experiments involving the use of electroshock or drugs, the vast majority of situations producing convulsions in the rat have included some form of relatively intense auditory stimulation. This is true even of certain investigations designed ostensibly to demonstrate the role of conflict (e.g., **92**) or dietary deficiency (e.g., **165**) in the production of seizures. In the early studies it was not clear which physi-

cal characteristic of the sound stimulus was primarily relevant. Maier and Glaser (**99**), finding jingling keys a more effective stimulus than the ringing of an electric bell, an air blast, or high pitches produced by a Galton whistle, even though to the experimenter's ear it was the least loud, concluded that "the degree of effectiveness depends more on the quality of the noise than upon the intensity" (**99**, p. 28). Maier (**95**) adds later that the optimal quality also varies from colony to colony. On the other hand, it has been shown that with sound frequency held constant, the number of seizures increases with increased intensity, while with intensity unchanging a positive relationship obtains between sound frequency and number of seizures (**121**). Carrying the analysis further, Morgan and Galambos (**120**) have demonstrated that the effectiveness of an auditory stimulus depends not upon its absolute intensity level, but upon the sensation level (i.e., decibels above the rat's threshold for the frequency involved), and that even low pitches, if of sufficient loudness, can produce seizures. It is therefore reasonable to presume that the greater effectiveness of the jingling keys was dependent upon the presence of overtones of high frequency, inaudible to the human ear but falling within the range of maximal sensitivity of the rat (cf. **49** for the rat's audibility curve). It is unfortunate that so few investigators have attempted any objective analysis of the sound pattern employed, but instead have depended for calibration upon the "experimenter's ears" (**97**, p. 285). This reduces the meaning of such statements as "the water test offered much less auditory stimulation than did the key jingling or air hiss tests" (**97**, p. 285), and vitiates the conclusion that in such instances the auditory component is a negligible factor. Similarly, it may be the uncontrolled variation of loudness, rather than increased conflict, that accounts for increases in frequency of seizure when the subject is tested in a discrimination situation (**92**) or in an enclosed testing chamber (**99**). It would seem necessary to assume for the present that the effect of sound depends predominantly upon its sensation level (for the rat), and that this characteristic must be controlled before less measurable aspects of the situation are considered causal.

It must be added that the intensity required to precipitate a seizure varies from individual to individual, and within the same subject from time to time. Greenberg and Tufts (**53**) point out that as the rat becomes more deficient in magnesium, a less intense auditory stimulus is required to set off the convulsion, and others have found similar results with subconvulsive injections of metrazol (**106**). It may be expected that a number of factors which influence the reactivity of the receptor and nervous systems will vary the effectiveness of a constant auditory stimulus (cf. many of the following subsections, and **75**, **92**, **99**, **122**, **123**, **127–136**).

The total energy of the auditory stimulus is not always related to

the likelihood of seizure. The number of convulsions is no greater with continuous sound than with alternating 1-second periods of sound and silence, although with longer cycles the interrupted tone becomes less effective (**45**). It may be significant from the standpoint of threshold mechanisms that the latency of attack is related neither to intensity (**119**) nor to temporal pattern of stimulation.

There has been no experimental demonstration that it is stimulation of the cochlear receptors that is a determiner of convulsive behavior. Because of the intimate anatomical relation between vestibular and auditory receptors, it is not impossible that the vestibular function is the more important here. This possibility is perhaps strengthened by the finding that swinging the rat during sound stimulation increases the number of seizures (**75**), by the frequent observation of peculiar circular or pivoting movements just prior to the attack proper and the occasional circular pattern of running, and by the depression of the righting reflex in the postconvulsive stage. Then, too, it may be the action of cutaneous or pain receptors, particularly in the middle ear, that is the really significant feature of the "auditory" stimulation. There is need for a more clear delimitation of the necessary stimulation modality, probably by pharmacological or operative techniques.

Electric Shock

Electric current has been used in several types of rat problem, sometimes as a motivator, sometimes as a punishment or pain-producing agent, sometimes to bring about specific physiological effects. The behavior evoked depends upon the intensity and duration of the shock, the point of application, and the condition of the animal. To yield a convulsive response one may, for example, pass a current of 600 v. and 4.5 ma. through the skull for an interval of 2.0 sec. (for other combinations cf. *supra*, Section II). Such stimulation usually leads to a tonic-clonic seizure of perhaps 30 seconds duration (**125**), followed by a comatose stage from which recovery gradually takes place over a period of several minutes. With a less intense shock, the tonic-clonic phase is often preceded by the disorganized running that characterizes the sound-induced seizure.

Heilbrunn (**66**) attempted to abolish the electroshock convulsion by drug injection just before stimulation. Results were negative with the following: atropine sulphate; synthetic vitamin K; calcium gluconate; thromboplastic suspension of brain substance, with or without calcium gluconate; synthetic vitamin K and the suspension of brain substance; and vitamin K, brain substance, and calcium gluconate. He was able to diminish the severity of the seizures, or to abolish them completely, by light ether anesthetic.

When the shock is applied to the rat via other pathways, such as the legs, convulsions do not normally result (**6**, **10**, **15**), although extreme

fatigue and passivity may supervene with continued stimulation (**30**) and fear and resistance to the situation may accompany repeated trials (**29**). When shock applied to the feet has been used to force the rat to respond in a difficult discrimination situation, convulsions have virtually never occurred (**10**, **56**, **92**, **123**). Only Griffiths (**56**) has been able to produce convulsions in such a situation (one of a group of 10 rats), which, it has been suggested by Stainbrook (**157**), might have been actually the result of cerebral electroshock. The fact that mild peripheral shock is one of several stimuli which have been successfully applied to evoke convulsions in animals with a variety of dietary deficiencies (**50**; but also cf. **135**) suggests an alternative explanation of Griffiths' findings.

Electric shock has been coupled with auditory stimulation in several experiments. Page (**125**) found that the convulsive threshold to cerebral electroshock was not altered when the rats were tested "while they were exposed to the sound of air blasts" (p. 186). Maier and Glaser (**101**) electrified the floor of the test chamber every 2 seconds while susceptible rats were being stimulated by the sound of jingling keys. The number of responding animals and the total number of seizures were essentially the same as for control periods of sound stimulation alone. Morgan and Waldman (**123**) likewise found that the addition of shock to a sound plus conflict situation had no appreciable effect on the number of seizures. The results of Bitterman and Warden (**15**) seem to contradict these observations, for the distraction of the shock decreased the number of seizures exhibited by their rats. However, the results of their control trials reduce the significance of these findings.

Other Stimuli

There has apparently been no demonstration that other exteroceptive stimuli are capable of eliciting convulsive behavior, except in conjunction with certain factors of an atypical nature. Unusual sensitivity to noise is reported by Chick, El Sadr, and Worden (**17**) in rats reared on a diet low in vitamin B_6: "the slightest external stimulus seemed to precipitate" seizures quite similar to audiogenic seizures (p. 579). In some of these rats immunity to noise developed, but seizures could still be induced by stroking, by pricking the tail, or by placing the animal in an unusual position. Similar observations are reported by Patton, Karn and Longenecker (**134**). J. R. Hamilton (**65**) describes convulsions lacking the running component that have appeared over a period of several years in one laboratory strain of rats. Noises may precipitate the attacks, but tactile stimulation is most effective. On the other hand, stimulation of an intense nature inhibits these seizures.

Other forms of stimulation have been found to affect the capacity of sound to induce seizures. Humphrey and Marcuse (**75**, **76**) present

evidence that swinging or rotating the animal during sound stimulation may increase its susceptibility to seizure, and Maier and Feldman (**97**) have demonstrated that a water spray has a similar effect. In the opposite direction, immersion in warm or cold water may reduce the number of sound-induced convulsions (**101**), although the significance of this experiment is obscured by concomitant exercise of a strenuous nature. Stimulation by bright light is also said to inhibit seizures (**92**). Whether the modifying effect of these several stimuli is mediated by the production of changed emotional status or operates in a more direct fashion cannot be decided on the basis of the experimental data now available.

Age Differences

With increasing age, a number of vital physiological alterations take place in the organism. Which of these changes modify the responsiveness of the rat to sound-induced seizures is not known,[7] yet there is no question but that the degree of susceptibility is in part a function of age (**27, 31, 39, 102, 115**). The number of responding rats in a colony seems to increase during the first months of life, reaching a maximum at about 135–150 days and declining thereafter. There are, however, individual exceptions to this trend, with some rats (maintained on standard colony diet) first exhibiting convulsive behavior at 250 days or later. Although a decrease in susceptibility is the normal accompaniment of aging beyond about 150 days,[8] some animals continue to demonstrate the full pattern at 500–600 days. The question of the relation between severity of attack and age is not clearly settled. In younger rats the seizures seem to be principally of the running type, with the more severe convulsions appearing more frequently with age increment (**34, 130**). Smith (**153**), however, reports more deaths during seizure in the younger subjects. According to Maier and Glaser (**99**), rats over seven months of age have fewer attacks of the "extensor type" than do younger rats.

The age of the rat at the time when withdrawal from or supplement to the diet is made is of importance in influencing the occurrence of sound-induced or "spontaneous" seizures (cf. *Nutritional Factors* below). A diet lacking in pyridoxine may fail to produce convulsions in a mother, while the offspring will exhibit severe disorders during the nursing period (**134**). Too, it appears likely that the earlier in develop-

[7] Alterations in the rate of metabolic processes, the changing endocrine balance, the number of intestinal flora capable of synthesizing vitamin B (134), the atypical heart rate development of the rat (111), and the variations in auditory sensitivity might be relevant.

[8] Farris and Yeakel (33) trace a decline in the susceptibility of albinos beginning as early as 60 days. The exact time relations will undoubtedly vary from investigation to investigation, depending on such uncontrolled factors as diet, strain of rat, mode of testing, etc. For example, the same experimenters (34) find that in gray Norway rats the percentage of attacks increases up to the age of 80–100 days.

ment a supplement of vitamin B-complex concentrate is given, the more complete the protection against sound-induced seizure (**128**).

Inasmuch as susceptibility has been shown to vary markedly with age, and in a highly individual manner, it is essential that particular effort be made to control this factor in investigations attempting to ascertain the effect of other variables upon seizures, especially where a wide age range is to be covered.

Sex Differences

There is little clear-cut evidence concerning sex differences in susceptibility to convulsion. Morgan (**119**) found no reliable difference in the average latency of audiogenic seizure between the two sexes, and no statistically significant differences in over-all incidence has been reported (e.g., **7**). But in spite of the lack of conclusive data in any single study, the trend of all available figures is in the same direction—slightly more males than females respond convulsively during auditory stimulation. Unpublished data of the writer show 18.7% of 268 unselected males and 11.6% of 112 females susceptible over a series of four tests, Maier and Glaser (**102**) report 55.7% vs. 52.3%, and Farris and Yeakel (**34**) and Finger (**39**) found differences of a similar order. Farris and Yeakel (**33**) suggest that males may react more frequently early in life, with responses of females predominating later. Although females might logically be expected to be more variable in their response because of their periodic physiological changes, it is impossible without further experimental data to assign greater total susceptibility to either sex.

Differences in Genetic Background

Strain differences. A number of workers have noticed differences in convulsive responsiveness among rats of different strains. It seems quite clear that domesticated gray Norway rats are much different from albinos in their response to sound (**31, 115**). In the first place, their startle is appreciably greater (**36**). Then, the number of animals susceptible is significantly higher, in one instance exceeding 95% while a comparable group of albinos yielded less than 70% of susceptibles (**34**). The average percentage of trials per rat giving seizures is higher for the gray Norways, the increase with age continuing longer and the subsequent decrease postponed several weeks. Finally, more seizures among the grays, both relatively and absolutely, include the convulsive phase, as contrasted to the predominance of running among the albinos tested with the same auditory stimulus.

The results with wild gray rats are decidedly in contrast. Griffiths (**58**) was not able to induce a single seizure in 126 wild Norway and 15 wild Alexandrine rats, using air blast, jingling keys, and Galton whistle successively as sources of sound. Unless these negative results can be attributed to an insufficient intensity of stimulation, we must

explain the difference between the responses of the wild and domesticated groups in terms of such uncontrolled factors as age, dietary background, and emotional state.

Differences in inherited "emotionality." A second genetic difference influencing susceptibility to sound relates to Hall's concept of emotionality (**61**). His primary measure of emotionality is the amount of defecation and urination displayed by the rat when placed in an unfamiliar open field of 8-foot diameter. The tendency to react in this emotional fashion seems to be inherited (**62**), and rats bred from a so-called emotional strain display fewer convulsive attacks in response to sound than do rats from a nonemotional strain (**11**, **113**, **114**; cf. also *infra*, p. 223).

Differences in familial susceptibility. The greater part of the evidence points to the conclusion that, with other factors held constant, rats with susceptible parents are more susceptible, on the average, than are offspring of nonsusceptibles. For example, by selective breeding over a number of generations, one strain can be produced with a relatively higher degree of susceptibility, a second with a much smaller percentage of convulsions (**57**). The precise mechanism of genetic transmission is still in doubt, however. Dice (**27**) and Watson (**168**) concluded that an "epileptic" pattern in mice, apparently somewhat similar to the sound-induced seizure in rats, was transmitted as a Mendelian recessive trait. Maier and Glaser's early study (**100**) concluded that the susceptibility to convulsion behaved as a simple Mendelian dominant, but the data of Maier's later experiment "do not support a single Mendelian type of transmission" (**95**, p. 331). The entire hereditary background of a particular rat must be considered in predicting his susceptibility: 68.6% of five litters with nonsusceptible parents and grandparents, but from an "unstable strain," were found to convulse to sound (**95**; cf. also **57**). The source of much of the confusion concerning this matter arises from the neglect of other relevant factors, such as age. As has clearly been shown, the diagnosis of "nonsusceptible" made on the basis of tests over a limited span of life is misleading, for a rat failing to respond at the age of 20 weeks may respond for the first time at 40 weeks (**39**). Certainly the question of genetic transmission will not be settled until more investigations are made, with systematic control of the other pertinent variables.

A little information is available indicating that convulsive response to metrazol is related to the inherited susceptibility to sound alone (**95**). This may be further evidence of the unity of the several "kinds" of convulsion in the rat; additional study may reveal the inheritance of a constitution with a certain level of general convulsion-tendency.

Differences in "nervous tone." In one group of rats having experienced a number of audiogenic seizures, the systolic blood pressure was found to be abnormally high (**115**). Inasmuch as the majority of these animals were from a single litter, the experimenters hesitated to ascribe

the hypertension to the occurrence of seizures. Rather, "audiogenic seizures may be simply a measure of that factor in the animal's constitution ('nervous tone') which causes the hypertension" (p. 303).

Differences in "Emotionality"

As indicated above, rats from a strain bred for "emotionality" (as measured by response in the open field test) are less susceptible to convulsion when exposed to sound stimulation than are rats from a nonemotional strain. When the rats themselves are tested for emotionality in the open field, a higher percentage of those classified as nonemotional are convulsers, the correlation between the two patterns of behavior being —.57 (**11**, **113**). It has been suggested that the chronically emotional animals have an habitual "safety valve" which allows the constant dissipation of tension rather than its building up to the threshold of convulsion (**114**). Requiring a somewhat different interpretation is the discovery that "gentled" rats have fewer sound-induced seizures than do untamed rats (**76**, **110**).

Conflict and Chronic Disturbance

It would be difficult indeed to define "conflict" to the satisfaction of all psychologists and in such a manner that there would be unanimity as to the conflict value of a particular situation. Eschewing this task, I shall simply quote the word as it is used in certain experimental reports; some readers will desire to insert the modifier "alleged" at several points. The controversial literature may shed additional light on the semantics of the situation (e.g., **13**, **40**, **41**, **56**, **92**, **96**, **118**, **123**).

It has already been noted that a few investigators impute conflict to every situation in which an intense auditory stimulus evokes convulsive behavior (e.g., **13**). From this standpoint, the material reviewed above under *Auditory Stimulation* becomes immediately relevant, with the substitution of "conflict" for "sound." Beyond that, there are a number of examples in which a factor termed "conflict" has been examined as to its effect upon convulsive behavior. It would be very convenient for the student of conflict, incidentally, if sound-induced seizures *were* demonstrably independent of conflict. In such an instance, the facilitating influence of deliberately provoked conflict upon a subthreshold convulsive tendency (with a standard auditory stimulus) could become a measure of the degree of the conflict itself. The sound-stimulated rat would then become an assay object for conflict just as it might for an inhibiting or facilitating drug. And similarly for several others of the factors considered in Section IV.

As has been pointed out, Griffiths (**56**) describes convulsions in a rat forced by an electric current to choose between two negative cards in a Lashley jumping apparatus. The dominant factor as explained by the experimenter is stimulation of the emotional centers, with "Fear and

escape . . . the basic principles underlying the seizures" (p. 29). The proponents of the conflict hypothesis on the other hand could describe the crucial factor as being conflict between the necessity of reacting and the unavailability of an appropriate response.[9] No other example of convulsion resulting from non-auditory conflict is in the literature.

Conflict brought about by the presentation of a difficult discrimination problem has several times been reported at least to increase the likelihood of seizure in response to sound. Maier and his collaborators (**92**, **99**, **103**) found that a greater number of seizures result when the animal is faced with a one-window situation in the Lashley jumping apparatus and at the same time stimulated by an air blast than when stimulated by air blast alone. The greater number of seizures is attributed to the addition of the conflict based on the jumping problem. (On the other hand, it is interesting to note that in the conflict-plus-sound situation the percentage of *convulsions*, contrasted to simple running attacks, may be smaller than in response to sound alone.) Using a small number of rats in a similar experiment, Morgan and Waldman (**123**) obtained negative results.

Other features of the testing situation are said to add to the conflict and thereby increase the number of seizures in response to sound. Erecting walls on the testing platform produces more convulsive seizures, and enclosing the subject on all six sides leads to an additional increment (**99**). Whether the greater effectiveness of this arrangement is due to alterations in the auditory component or in the conflict aspect is still debated. When a rat stimulated by sound is simultaneously stimulated by a powerful water spray, more convulsions occur than when the non-auditory effect of the spray is lacking (**97**). These results have been interpreted as meaning that conflict between the need to escape the annoying spray and the inability to do so increases susceptibility to convulsion. That the facilitating effect of the water depends on more than its cutaneous stimulation value is yet to be established.

Two other experiments may be interpreted as supporting the contention that the incidence of sound-induced convulsions can be increased by conflict. Bernhardt, Tobin and Signori (**10**) were unable to produce convulsions in a difficult Lashley discrimination situation even though response was forced by electric shock. When rats exposed to this problem were subsequently tested with a loud bell, however, they exhibited more seizures than did a control group lacking discrimination training. Humphrey and Marcuse (**75**, **76**) produced "chronic disturbances" in rats by noisily moving the bottomless feeding box after completion of maze trials. The animals so treated convulsed more readily in response to noise than did a control group. Small numbers and incomplete controls diminish the significance of these experiments, however.

[9] Alternative explanations are mentioned above.

Anger and Fear Reactions

Tseng (**164**) reports that the induction of a prolonged anger state by teasing increases the likelihood of a rat's convulsing upon exposure to loud sound. In the same laboratory, Arnold (**2**) observed that on trials in which "fear" reactions (tremor, twitching, lip-wetting) occur there is decreased probability of seizure. That injections of adrenin reduce susceptibility is presumably related to this finding; the adrenin induces the fear symptoms, which becomes a substitute for convulsion.

Other Behavioral Relationships

Substitute activities. If under "convulsion" is subsumed only the running-tonic-clonic pattern, then an inverse relation obtains between the appearance of convulsions and certain other forms of response to sound. In nonsusceptible rats, or on the nonseizure trials of susceptible rats, the onset of the auditory stimulus is frequently followed by face rubbing, ear scratching, body cleaning, sniffing, or chattering (**2**, **56**, **88**, **97**, **151**). Inasmuch as seizure virtually never ensues if such forms of behavior occur, they may be classified as substitute forms of response—in one sense, a means of protection against convulsion.

Other forms of alternate activity have been described as characterizing non-convulsers: "haphazard" movements (**4**), increased motor activity such as climbing (**10**), exploration (**2**), attack of the sound source (**58**), general "nervousness" (**87**, **92**). Bitterman and Warden (**15**) point out that adding mild shock to sound stimulation not only produces more activity in the rat than results from sound stimulation alone, but also is followed by fewer convulsive attacks. It has been suggested that such forms of behavior, as well as chronic emotionality (**11**), fear reactions (**2**, **3**), and forced exercise (**76**, **101**), drain off the excitation provoked by intense stimulation before it can build up to an explosive level (**161**). Yet to be discovered are the factors which predispose an animal to these responses rather than to seizure,

Escape reactions. When the rat is forced (e.g., by an air blast) to make difficult discriminations in the Lashley apparatus, the well developed response is often disrupted by the appearance of position habits and inaccurate ("abortive") jumping. In the former, no attempt is made to select the correct stimulus card; instead, the jump is always to the same side. In the abortive reaction, the animal gradually loses his skill in reaching the window, hitting above, below, to one side, or even falling short of the food platform. These responses appear particularly when the negative stimulus is presented, and ordinarily can be eliminated by retraining. Maier (**93**) and Maier and Klee (**103**) report that the number of seizures produced by conflict plus sound diminishes when an animal develops either of these modes of jumping. The explanation suggested is that these reactions offer a means of escape from

the problem situation. An animal for whom the situation no longer involves choice (fixated position habit) or for whom punishment for incorrect choice is decreased (abortive jumping) experiences less intense conflict, and so, granting the facilitating role of conflict or emotional upset, would be expected to exhibit fewer convulsions.

The possibility of escape of another sort was provided in a sound-stimulation experiment lacking the discrimination problem (**56**). In this situation, the rat was exposed to the loud sound of a bell and during the running phase of the seizure was given the opportunity to run into a small "rat-hole." It is reported that of the rats entering this shelter, fewer continued into the convulsive stage of the seizure than had done so in trials prior to the introduction of this feature. The results are interpreted to mean that the rat's "affective tone" is a factor in determining convulsive behavior, and that the "psychological shelter" response makes the badly frightened rat feel more secure. Evaluation of the observations is difficult without further information regarding the control of such factors as sound intensity and adaptation.

Activity and restraint. Numerous observations have been made which relate the probability of sound-induced seizure to the amount of gross movement allowed or required during or just preceding the stimulation. If the animal is forced to move vigorously during this time, the convulsive responses decrease in number (**76, 101**) or are inhibited completely (**94**). On the other hand, placing the rat in a relatively small cage, so that movements tend to be restricted, is reported to increase the percentage of seizures (**99, 101**), although the meaning of these data is somewhat obscure because of small numbers and limited controls. This trend apparently breaks down upon increasing the degree of restraint still further, for binding the subject during auditory stimulation reduces (**72, 111**) or eliminates (**56**) the appearance of seizures, as well as certain physiological manifestations of abnormal response (**88**). It may be that while the relative restraint of a small cage reduces neuromuscular activity, the restraint of close binding actually increases neuromuscular activity in the form of isometric contractions against the bonds, thus providing as much of a "safety valve" as does forced exercise or substitute behavior. It would be enlightening to determine if habituation to such restraint leads to the reappearance of convulsive symptoms.

Nutritional Factors

The precise influence of nutritional factors upon convulsions induced by electroshock, sound stimulation, or conflict is difficult to determine by the average psychologist because of his lack of technical knowledge concerning the laboratory diet. It is often assumed that Purina chow, perhaps supplemented occasionally by a ration of spinach, is adequate for maintaining the rat at his physiological optimum, and

that for this reason the dietary factor can be disregarded as a variable relevant to convulsive behavior. There is accumulating, however, a formidable array of data on the effect of either extreme of dietary completeness: convulsions are very readily induced when certain constituents are minimal, and become increasingly difficult to provoke with the provision of generous supplements. As the influence of the extremes becomes more obvious, one is led to suspect that in the intermediate condition—that of "standard" colony feeding—the nutritional state is an omnipresent determiner. It would appear that more detailed examination of this factor is justified, extending even beyond studies of convulsions to such problems as learning and motivation.

Magnesium. Some fifteen years ago it was reported (**85, 124**) that rats on a low-magnesium diet frequently exhibited convulsions outwardly similar to the seizures later described as "neurotic" or "audiogenic." Further experiments (**50, 51, 52, 53, 135, 136, 165**) have amplified our knowledge of this phenomenon. If a group of rats, free of convulsive responses even to intense sound, are put on a diet which includes only 1-2 mg. of magnesium per 100 g. of dry food, the plasma and body magnesium levels drop, vasodilatation is observed for a few days, and then a state of hyperexcitability begins. In this condition, all rats will exhibit the running-tonic-clonic pattern when stimulated by sound (but not when such stimuli as shock or light are used). At this stage the replacement of magnesium in the diet alleviates the symptoms in most of the animals (**136**). When smaller amounts are included in the diet (e.g., 0.4 mg. per 100 g. of food), the convulsive behavior may be induced sooner, is somewhat more severe in form, eventually comes to occur in the absence of any demonstrable stimulus, cannot be cured by the addition of magnesium salts, and often terminates in death (**53**). With 5 mg. or more of magnesium per 100 g. of food, some rats exhibit the convulsions only under special conditions, e.g., when lactating or when calcium is added to the diet. Supplements of vitamin G delay the appearance of the syndrome (**53**).

Vitamin B. In 1938, Kline, Tolle and Nelson (**84**), employing the rat as an assay object for vitamin B_1, described a polyneuritis in rats maintained for 25-50 days on a diet deficient in this vitamin. In the early stages "a state of extreme contraction of the musculature" was observed; later, "the animal . . . [exhibited] almost continuous uncontrolled movements" (p. 309). A 2-unit administration of the vitamin caused the symptoms to disappear within 24-48 hours. More closely resembling the sound-induced convulsive pattern was that observed in 1940 by Chick, El Sadr and Worden (**17**) in rats deprived of vitamin B_6. The seizures often occurred daily, and "the slightest external stimulus seemed to precipitate them" (p. 597). Cure was effected by the addition of B_6 in adequate amounts. A number of later studies (**19, 24,**

59, 127, 132, 133) have verified that the lack of normal B-vitamin content in the diet brings about susceptibility to sound-induced convulsions, and that in extreme cases "spontaneous"[10] seizures occur that are indistinguishable in form from the audiogenic seizure. Conversely, almost complete protection against audiogenic seizure is afforded by the inclusion in the diet of large amounts of vitamin B, although the supplement is maximally effective only if provided early in life.

There are a number of complications that make difficult an exact statement of the role of the vitamin B complex in this behavior. Hunger (**38**, **121**) and inanition (**81**, **132**) themselves affect susceptibility to sound-induced seizure (each function passing through a maximum), and a subnormal vitamin level usually decreases food intake. Further, the effects of inanition and vitamin deficiency may cancel each other out when they are present simultaneously (**131**). Multiple vitamin deficiencies may similarly counteract each other, and supplementary feeding with other vitamins at times increases the severity of attack in B_6-deficient animals (**134**). Finally, restoration of food after a period of fasting leads first to an increase and then to a decrease of seizures (**132**). But certainly it appears that the dietary factor is an extremely important one in the determination of convulsions, and may account for the differences in susceptibility accompanying differences in age, strain, and colony background. The importance of this variable in unsuspected cases is enhanced by the observation that a deprivation which has no effect upon adult females may still induce susceptibility in their suckling young (**24**, **128**, **134**).

Other constituents. Other nutritional factors have been investigated for their effect upon response to auditory stimulation. Rats maintained on a vitamin D-deficient diet for the first four weeks of life were no more susceptible to sound than were a group given the standard laboratory diet (**153**). Increments in frequency of convulsive attack may result from parathyroidectomy and low calcium intake (**50**).

Although the mechanisms by which nutritional factors modify convulsive susceptibility are not yet known, it is clear that their influence upon the rat's physiological condition must constantly be

[10] Whether or not an auditory stimulus is essential as a precipitant of convulsions in the malnourished rat is not clear. Patton, Karn and King (**133**) observed that the onset of seizures is often delayed until 1–5 min. after termination of the auditory stimulus, which may account for the "spontaneity" of some cases. Only the deafening of rats exhibiting allegedly spontaneous seizures can rule out this type of etiology, or strengthen the unique position of the auditory modality in the production of seizures. It may be relevant to note that Galambos and Davis (**44**) found about half the fibers in the cat auditory nerve firing "spontaneously" even when precautions had been taken to eliminate ambient noise. Incidentally, the overt manifestation of seizures in the discrimination situation has occasionally been observed to have its onset several seconds after the termination of the auditory stimulus (**93**).

considered by the investigator of seizure behavior. The manipulation of this variable gives promise of yielding results of broad significance.

Drugs

Allied to the modification of physiological condition through control of diet is the injection of drugs as a means of altering seizure susceptibility. There are a number of drugs, notably metrazol and insulin, which have been used as the precipitant of convulsions in the rat (**68**, **80**, **141**, **142**, **174**; cf. also Section II). The pattern of this response differs from that involving intense sound or low intensity electric shock chiefly in its lack of a running phase and, with metrazol, the prominence of its characteristic spinal torsion.

Metrazol, when given in subconvulsive doses, so affects the rat that an auditory stimulus otherwise ineffective becomes adequate to evoke a prompt seizure (**56**, **78**, **106**, **145**, **154**). Such an attack includes a running component, and in this way resembles the usual sound-induced pattern more closely than it does the metrazol convulsion, although its severity is often greater (**78**). The tonic flexion typical of the drug convulsion is observed only occasionally with this combination (**106**). Other drugs successfully utilized to lower resistance to sound include strychnine (**2**, **153**, **154**), caffeine sodium benzoate (**154**), sodium phosphate (**50**), coramine (**166**), eserine in adequate dosage (**72**, **73**), nicotine (**72**, **74**), and perhaps sodium bromide administered prenatally (**64**).

A few substances reduce susceptibility to sound-induced seizures: adrenin (**2**, **87**), dilantin sodium (**21**, **54**, **148**), adrenal cortex (**54**), atropine (**72**, **73**), and small doses of eserine (**74**). Rats rendered susceptible to sound stimulation by strychnine injections are protected from convulsion by adrenin (**1**), but the simultaneous use of atropine and eserine does not yield a similar neutralization (**74**). Semiparalyzing dosage of curare appears to abolish the susceptibility of rats on normal laboratory rations (**88**), but those maintained on a low-magnesium diet can still exhibit seizures under the influence of subparalytic amounts of curare (**53**), as do thyro-parathyroidectomized rats on a low calcium diet (**50**). Phenobarbital (**77**), mecholyl, benzyl benzoate, and pituitrin (**74**) fail to affect significantly the response to auditory stimulation.

Convulsive threshold to cerebral electroshock is uninfluenced by adrenin (**125**), atropine, calcium gluconate, thromboplastic suspension of brain substance, and vitamin K (**66**). The susceptibility is perhaps lowered slightly by 1.0 cc. injections of alcohol (**125**), and light ether anesthetic eliminates the convulsions (**66**). Animals rendered sensitive to sound by magnesium deprivation are protected temporarily by sodium amytal, while the midbrain drug picrotoxin perhaps enhances the probability of seizure (**53**).

In outlining the convulsant properties of drugs, it is always essential

to add that the "typical" effect of a particular drug can be altered by a great number of characteristics of the organism and of the external situation (**69**). These additional factors must be taken into account by the investigator adopting the pharmacological approach to convulsive behavior.

Previous Auditory Stimulation and Convulsion

It is seldom possible to elicit the full convulsive pattern by renewed stimulation directly following a seizure induced by auditory excitation. Certain exceptions to this trend appear, and in some cases of severe dietary deficiency a series of repetitive convulsions resembling status epilepticus may be observed. (As has already been pointed out, no such refractory period follows the electroshock convulsion.) In general, however, it appears that immediate susceptibility is greatly reduced by convulsion, and even by auditory stimulation unaccompanied by seizure (**101**). But the various investigations of this adaptation process as it extends over longer periods of time, involving repeated trials, yield divergent results. Several experimenters (**11**, **64**, **76**, **153**) find that with repeated stimulation, usually at 24-hour intervals, the likelihood of the rat's response becomes progressively less, and that a vacation from stimulation restores the original level of response. On the other hand, Patton and Karn (**130**) and Parker (**126**) found little evidence of adaptation, even when the daily trials were continued for 30 days, and Griffiths (**56**) found that repeated stimulation actually made the animals less resistant to convulsion, as indicated by a shortened latency and increased duration of attack. Using the air blast as stimulus, Maier and Glaser (**99**) found a gradual decrease in number of attacks, but with jingling keys as the source of stimulation there was little decrement in frequency. These seemingly contradictory findings may be reconciled if it is assumed that a rat possesses a threshold of susceptibility which is a function of a number of factors and which is raised temporarily as the result of convulsion or stimulation. If adequate time elapses before the next trial to permit recovery of the threshold to its original level, a second response can occur. If, on the other hand, the second test is given before the threshold has returned to its former level, the convulsive response may fail to appear. This presumption is confirmed by the observation that animals regularly responding at 24-hour intervals fail to respond when the trials are given at shorter intervals (**38**). Of course, even when the recovery is incomplete, a more intense stimulus may nevertheless induce seizure. It is this possibility that explains why in one testing situation adaptation takes place while in another it is not evident; the rats in Patton and Karn's and Parker's experiments may have been stimulated more intensely than were those animals demonstrating adaptation, and Maier and Glaser's key jingling may have provided more effective (high frequency) stimulation than did their air

blast. In summary, the probability of a rat's response depends not only upon such factors as his genetic constitution, age, emotional state, and dietary background, but also upon the interval since the last testing, the intensity of the auditory stimulus, and his individual rate of recovery. Why it is that threshold should vary with experience is a challenging problem for the neurophysiologist.

Neural Ablation

One of the most obvious techniques by which the nature of convulsions can be studied is operation on the nervous system. The role of the auditory receptors, the participation of the cerebral cortex, the influence of lower brain centers, autonomic involvement—these are theoretically open to surgical investigation. Unfortunately, technical difficulties exist which have tempered the enthusiastic adoption of this approach, and as a consequence little information is available.

Cerebral lesion. Maier's original report (**92**) included three cases in which convulsive responses to difficult discrimination plus air blast were observed in spite of partial cortical extirpation (15–20% of the frontal cortex in two rats, and 25% of the median region in the third). According to Smith (**153**), "certain results suggest that removal of the frontal pole of the cortex abolishes the convulsive seizures" (p. *326*). Two experiments have attacked the problem directly with conflicting results. Beach and Weaver (**7**) subjected the three most susceptible of a group of 12 rats to two operations during which was removed more than 90% of the neopallium, as well as most of the corpus callosum, much of the hippocampus, and parts of the corpus striatum. When tested about two weeks later, the decorticates responded on each of 12 tests to jingling keys, as contrasted with 91% of 42 preoperative tests. Unfortunately, the control animals had also increased in susceptibility during the interval, responding 79% of the time as compared to the previous 36% positive trials. In each group the proportion of seizures which included the tonic-clonic phase increased. Weiner and Morgan (**169**), on the other hand, found that while the percentage of seizures in their surgical control group did not change significantly, there was a decrease in the number of seizures exhibited by the 20 experimental animals. Among those suffering prefrontal lobectomy, the number of responses dropped from 22 to 10; in those with lesions in the auditory area, there was a reduction from 41 to 4; those with motor area lesions had 10 as contrasted with a previous 30 attacks. It is significant, however, that only one seizure in the prefrontal group included the tonic-clonic component of the convulsion, while all seizures appearing in the other operated groups were of the full convulsive type. No change in average latency of seizure was found, in contrast to the marked decrease reported by Beach and Weaver.

To reconcile these divergent results seems impossible. The differences in number and strain of rats, original level of susceptibility, operative recovery period, and extent of destruction qualify any direct comparison of the two experiments. Further definitive work will be necessary.

Vagotomy. The possible mediation of convulsions by the autonomic nervous system suggests operative limitation of its function. It seems on the basis of

preliminary data that vagotomy decreases (**72**) or abolishes (**88**) the appearance of seizures.

Other Factors

There are a number of minor factors which apparently contribute to individual differences in susceptibility. Some are subject to experimental manipulation, and others can be controlled only statistically. A full explanation of convulsive behavior must take them into account.

Blood composition. The relationship between plasma magnesium level and convulsion has already been noted (**51**, **165**). Lacey (**86**) found that the serum protein level of rats responding convulsively to the buzzer and air blast was higher (significant at the 4% level) than that of nonsusceptibles and suggested neural dehydration as the mediating mechanism (cf. **38**). Further, the susceptible group was characterized by the greater variability in blood sugar, perhaps relating to a dysfunction of the sympathetico-adrenal system.

Endocrine action. Little work has been done relating the endocrine balance to seizure threshold. Removal of the thyroid and parathyroid glands, coupled with low calcium diet, tends to increase susceptibility to stimulation-convulsion (**50**). No effect of oestrous cycle was observed by Mirsky, Elgart and Aring (**117**), which suggests lack of pituitary-ovarian influence.

Heart rate. In a group of rats selected for their convulsive response to air blast, Lindsley, Finger and Henry (**88**) found that in trials culminating in convulsion the initial heart rate averaged 379.4 per minute, increasing 20.1 beats per minute after the onset of stimulation, while in nonseizure trials the initial rate was 398.2 and the average increase only 4.4. These intertrial differences were not statistically reliable, however.

Startle response and general activity. Turner (**167**) found no difference between susceptibles and nonsusceptibles with respect to diurnal activity pattern. The startle response of the susceptibles was clearly the greater, however, as was the amount of their activity during brief periods of moderate noise.

Learning ability. In an exploratory study of the relationship between learning ability and seizure susceptibility, Sisk (**152**) gave 35 rats 14 daily trials on a 15-unit T-maze and then tested their convulsive reaction to the sound of a bell. The six responding positively to the noise had exhibited an average maze performance slightly inferior to that of the nonsusceptibles, but the difference was too small to be more than suggestive.

Physical condition. Farris and Yeakel (**34**), finding that rats tested in the spring and summer reacted more violently to sound than did those tested in the winter, have suggested that the significant variable is general physical condition: the more susceptible rats were from litters born in late fall and winter, and typically less sturdy. (Cf. also **95**, **131**, and *Nutritional Factors* above).

Body temperature. There seems to be a tendency for the lowering of body temperature to decrease (**97**, **101**) or abolish (**94**) seizures in response to sound, although the experimental demonstrations are somewhat complicated by other factors.

Combinations. In an attempt to establish physiological patterns distinguishing between rats responding convulsively to noise and nonsusceptibles, Marcuse

and Moore (**110**) compared measures of their retiring behavior, sensitization, weight, frequency of defecation, and cardiac action. By using a composite criterion including at least two of these indices, "... a group was selected which comprised 70% of the susceptibles and 18% of the non-susceptibles ..." (p. 2).

V. The Effect Of Convulsions

Because of the possible applications to problems of human shock therapy and epilepsy, a number of observations have been made of the effect of convulsions upon other behavior patterns and the structure of the rat. Most of the specifically planned experiments have been concerned with learning and retention, but incidental observations are recorded regarding several other points.

Learning Ability

The few experiments in which the effect of convulsions upon learning has been investigated point generally to a slight decrement in acquisition ability, especially as measured by speed of response (**16, 71, 141, 149**). However, the differences are so slight, the scope of the research so limited, and the operation of distorting factors so probable (e.g., motivation, **6**, and emotional state, **149**), that this conclusion can be presented only very tentatively. The results obtained in future experiments will undoubtedly depend upon such variables as the kind of convulsing situation employed, the age and physiological condition of the rats, the number and spacing of the seizures, the difficulty of the learning problem, and the interval between the convulsion series and the learning test.

Retention

The disrupting effect of convulsion upon material already acquired has been subjected to somewhat closer scrutiny. Four investigators have employed loud sound to induce the convulsions. Three experiments (**55, 151, 160**) revealed no appreciable loss of a maze habit after 1-3 convulsions, although there was in some cases a tendency for the time of running to increase unless some form of forcing was adopted. In one experiment (**108**) rats that had had 1-5 (?) seizures in the food box of the maze made significantly more errors on relearning trials than did those animals stimulated similarly but having had no seizure. The experimenter, however, felt that the deterioration in maze performance "was a product of the superposition of fear on a learned pattern, rather than a loss of a learned pattern" (p. 34). Taken together, these results

suggest that although there may be a change in general behavior as the result of a limited number of sound-induced seizures, there need be no disorganization of recently learned maze habits. Of course, there is no reason to assume similar stability of response following a longer series of convulsions.

The deleterious effect upon retention of even one or two convulsions induced by metrazol or insulin seems quite definite. In only one investigation (**68**) did the convulsed animals respond as well upon retest as did the control group, and the significance of this single finding is limited by the small number of convulsions involved (average of two). In other investigations involving maze behavior the seizures did lead to deterioration of the habit, particularly so with more difficult, less well learned, and more recently acquired responses (**9, 89, 141**). Insulin and metrazol convulsions have also been shown capable of effecting the recovery of an extinguished conditioned response (**46, 80**). On the other hand, there is no indication that either a fixation in the Lashley jumping apparatus or the well-learned discrimination habit itself is affected by metrazol convulsion (**82, 103**).

The results of experiments employing electroshock are similarly positive under certain conditions. It appears that very simple habits (e.g., a running response, a two-choice T-maze) may not be impaired, at least as measured by the number of errors on the retention trial (**149, 160**). However, on more difficult mazes and in one discrimination situation, retention scores have been lower, and certainly so if the number of shocks is increased (**29, 71, 156**). In at least one instance the deterioration of the pattern seemed to be due not just to emotional factors, but to a "direct effect on cerebral tissue" (**29**, p. 278). In still another experiment, an extinguished conditioned response reappeared after a single electrically-induced convulsion (**80**).

As in the series of studies involving learning ability, these investigations of retention are too limited in range to be conclusive. It appears that, given enough convulsions, there is measurable interference with certain habits. An extension of these experiments is sorely needed, with variation of difficulty and age of the pattern, number and spacing of practice trials, number and type of convulsion, retention interval, and method of retesting. And as Stainbrook (**158**) has intimated, it is always essential to differentiate between actual loss of habit (perhaps indicated by the criterion of errors or number of relearning trials) and the generalized effect of emotion or lack of alertness (as indicated by
attention? time measures). The obvious corollary of these studies is the investigation of the neurophysiological changes paralleling the behavioral modifications.

Reasoning Ability

Sharp, Winder and Stone (**146**) studied the effect of electroshock upon "reasoning ability" as measured by Maier's apparatus (**90**) which requires ". . . bringing together spontaneously two elements of past experience without having them previously associated by contiguity" (**91**, p. 46). The tests were given 1½ or 22 hours after electroshock convulsion. Although only six subjects were used and individual differences were apparent, the trend was in the direction of decreased distance in exploratory runs, increased time for test runs, and decreased accuracy scores. The impairment was greater 1½ hours after shock than when measured after 22 hours, indicating that at least part of the disturbance was of a transitory nature, similar in this respect to the effect appearing in retention studies.

Emotional Patterns

In the course of experiments testing learning and retention following convulsion, some observations have been made of changes occurring in other behavior patterns, particularly those described as "emotional." Stainbrook (**156**, **160**), for example, reports that after a series of electroshocks, an "experimental neurosis" appeared, during which the rats manifested excitement and resistance when approached, and appeared upset by slight noises. The condition persisted for several months. Page (**125**) found that while electrically convulsed animals resisted the experimenter during the first few days, they soon became passive and lethargic. In two experiments, one employing shock (**149**) and the other air blast (**108**), the rats seemed trying to avoid the situation in which the convulsions had been experienced, and evidenced emotional disturbance when placed in the situation again. Directly contradictory is the observation of Duncan (**29**) that animals having undergone 30 electroshock convulsions evinced no fear in the experimental situation, in marked contrast to rats shocked on the legs. Most experimenters employing loud sound as the convulsive agent similarly report no avoidance of the situation by the rat (e.g., **92**).

This survey of the available data makes it clear that too little is known about the generalized behavioral changes following convulsion in the rat. It is desirable that future experiments be oriented more deliberately and systematically in this direction, with objective measures adopted to reveal the possible changes.

Activity

No very refined technique is necessary to detect the decrease in activity immediately following convulsion. This relative immobility

varies in degree and duration from seizure to seizure, often retarding maze running for a period of several hours. Only three attempts at quantitative evaluation of activity changes have been reported. In the one involving sound-induced seizures (**42**) it was found that auditory stimulation without convulsion did not affect subsequent activity, but following a seizure the activity level dropped significantly. Recovery was apparently complete within ten hours, with no cumulative effect discernible after three seizures. Unfortunately, the number of attacks induced was too few to support any generalization regarding long-range modification of diurnal pattern.

It appears that a single electroshock likewise depresses activity measurably, and that during a period of daily convulsions the activity level drops significantly below normal (**162, 170**). The effect is not long-lasting, however; some recovery is evident in the first few days following termination of the series, and the preconvulsion level is approached within 2-4 weeks.

Mating and Maternal Behavior

Some of the early investigators reported difficulty in breeding rats shown to be susceptible to sound-induced convulsion (e.g., **100**). This might seem to imply that the convulsions themselves exert a depressing influence upon mating. The most direct test of this supposition is an experiment by Farris and Yeakel (**32, 35**). They found that animals subjected to air blasting five days a week beginning at weaning produced 30% more litters than did control litter mates not subjected to the sound. On the other hand, significantly fewer offspring of these experimentals than of the controls survived the first day of life, due in part to a noticeable inefficiency of maternal behavior among the experimental mothers (cf. **57**). Unfortunately, no breakdown of figures is given that would permit a separation of the influence of the auditory stimulation itself from that of the convulsions produced thereby.

Physiological Changes; Death

To the extent that behavior changes are found to result from the experiencing of convulsions, parallel changes in the structure or physiology of the organism might be expected. In general, no very striking alteration has been found, perhaps because relatively little attention has been paid the problem.

Rats (as well as other laboratory animals) undergoing repeated convulsions, induced either electrically or by loud sound, gradually deteriorate in general physical condition and gain less weight than do

control animals (**42**, **125**). It is possible that the systolic blood pressure becomes abnormally high (**115**). According to one exploratory study, exposure to sound, with or without seizure, causes the deposition of pigment granules in the adherent mucus of the stomach walls, but with no evidence of ulceration (**104**). Electroshock convulsion (and not the shock itself) produces hemorrhages in the pia-arachnoid and the parenchyma, presumably because of circulatory changes accompanying the convulsion (**66**). Finally, studies of the blood indicate that, following audiogenic seizure, "the gray Norway rat rapidly developed a shorter blood coagulation time, and an acidosis caused by acids other than carbonic in the blood" (**22**, p. 525). It will be noted that most, if not all of these physiological changes are of a temporary nature. Direct evidence of permanent modifications, e.g., in the central nervous system, is lacking.

The probability of death in convulsion apparently depends for the most part upon the age and physiological condition of the subject. Ordinarily, very few deaths occur during sound-induced seizure, although up to 4% mortality has been recorded among very young animals (**4**, **153**). In convulsions associated with nutritional deficiency, death seldom occurs if the deficit is moderate (**17**), but in extreme cases of magnesium deprivation death during convulsion, within 30 days, is the virtually invariable outcome (**53**, **85**).

VI. Physiological Bases Of Convulsions

When the psychologist is puzzled by the appearance of an atypical pattern of behavior, he often looks to the physiology laboratory for an answer. Or at least he remains uneasy until an answer to his "why" can be phrased in physiological language. An imposing array of behavioral observations confronts the student of rat convulsions, but the interrelationships of the numerous variables are so complex (and so frequently ignored experimentally) that a coherent picture is still lacking. Unable to systematize his behavioral findings at the gross level, the investigator searches for a description at the neurophysiological level, to which the diverse external manifestations may be referred. In short, the first five sections might be considered a mere preliminary to this section, which should be the climax of the review.

Unfortunately, however, no thoroughgoing theory of convulsion has been attempted, nor shall I essay one here. In reality, the prime purpose of this report has been to present the problem in such a way that the reader with an adequate background will undertake his own formulation. It may be of value to mention again some findings that

seem particularly deserving of attention in such a theory, and to add a few conjectures that have been made regarding them. But all this is intended only to hint at one direction which speculation may take, and is certainly too tentative to be a restraining influence on the flow of hypothesis. Even the contradiction of experimental data need be no final deterrent, for a great deal of the work demands repetition with more satisfactory controls.

It would appear that in the convulsions attributed to conflict, auditory stimulation, minimal electroshock, and nutritional deficit, the neural discharge finds a final path that is approximately common. If only the tonic-clonic stage were considered, this statement could also include certain drug-induced seizures. That this intense discharge is central in origin is suggested by several lines of evidence.

1. Electroencephalographic recording before and during audiogenic seizure reveals cortical involvement (88).
2. Cortical extirpation may decrease the probability of seizure to sound (169).
3. The mode of interaction of the convulsion-facilitating eserine and the inhibitory atropine indicates that their influence is through the central nervous system (73, 74).
4. Coramine, which increases the reactivity of the central nervous system, in subconvulsive doses increases convulsive susceptibility to sound (166).

But there is also reason to believe that subcortical and autonomic centers contribute to the innervation.

1. Cortical extirpation does not always abolish seizures (7).
2. Although the total number of seizures may be diminished by lesions in the motor cortex, the proportion of attacks which include the tonic-clonic phase is thereby increased. Weiner and Morgan (169) suggest that the normal function of this region tends to inhibit the more incoordinated (convulsive) discharge which has a subcortical origin.
3. According to Morgan (119) and Weiner and Morgan (169) latency of seizure is uninfluenced by a number of factors, including cortical extirpation, which affect threshold of seizure. This timing process may thus be a subcortical function. Relevant to this point is Smith's suggestion (153) of a physiological timing mechanism, cyclic in character.
4. Changes in heart rate during auditory stimulation are prognostic of susceptibility during that trial. Both these changes and the susceptibility to seizure are minimized by vagotomy (88).
5. In metrazol and electroshock convulsion the autonomic system is found to be stimulated, with the action of the sympathetico-adrenal system predominating (37, 80).

The puzzle is: Why do the central overloading and the convulsive synchrony of central firing occur? A number of possibilities may be mentioned.

1. Relative cerebral anoxia seems to accompany convulsion induced by electroshock and certain drugs (**25**, **79**). Such a condition may well alter the cell membrane surfaces so that the tissue becomes abnormally sensitive to stimulation.

2. In states of malnutrition a parallel sensitization might take place, so that normally ineffective stimuli would serve to cause mass response (cf. **136**).

3. In instances where these factors are usually absent, auditory stimulation might induce a temporary anoxia. Whether the mechanism involved should be termed reflex or emotional cannot be stated now.

4. Due to the proximity of the auditory and the motor centers, direct spread of excitation may be uniquely facilitated when this modality of stimulation is employed.

5. Hypothalamic centers may be excited rather directly, discharging both centrally and peripherally, via the autonomic nervous system (**88**). In the intact organism reverberation from the visceral organs may supplement the original cortical innervation, leading to explosive overflow unless constantly drained off by emotional (**2**, **114**) or substitute (**56**, **76**, **88**) reactions.

It is obvious that these suggestions are little more than isolated phrases in the physiological interpretation that finally must be made. More work, particularly involving electrical, pharmacological, and operative techniques, must be planned with neurophysiological hypotheses as the guiding orientation, at the same time recognizing the multiplicity of factors that must be segregated and controlled. It appears that breaking down the classificatory distinctions between at least some of the "types" of convulsion would facilitate this approach. What the final categories will be, and their relation to human paroxysmal behavior, must wait on a more complete experimental integration.

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A CRITICAL REVIEW OF THE LITERATURE ON "ABSOLUTE PITCH"

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Pitch discrimination and, more specifically, "absolute pitch" have been a source of controversy and speculation for a great many years, beginning with Stumpf's discussion of Mozart's sense of pitch (**43**). In general, absolute pitch is the ability of an individual to name correctly a particular tone that is sung or played on an instrument, without comparing it to any other heard tone. The explanations of absolute pitch have been varied and many. The inadequacy of these explanations creates our present problem. Even more so than other forms of abilities, musical talent, especially absolute pitch, has traditionally been explained as an inborn quality or faculty that relatively few people are privileged to have.

In view of the fact that an inborn quality has been accepted so generally as an explanation of accurate pitch discrimination, one might hesitate to offer any other explanation if it were not for the fact that no real evidence has been presented to prove that there is such a quality. Moreover, if all the work on pitch discrimination is reviewed, while keeping in mind the real event taking place in pitch discrimination, the evidence points strongly away from any faculty or quality inherent in the individual. Rather, the evidence might show that the ability to discriminate pitch depends upon the life conditions of the individual and is probably built up during his lifetime.

It is the purpose of this paper to criticize the definitions, theories, and experiments relating to absolute pitch, from an interbehavioral standpoint (**16**). This point of view stresses the behavioral event taking place when an organism is interacting with a stimulus object in any particular situation. The stimulus value that the object has for the individual depends upon the individual's life history, the situation at the time, as well as upon previous interaction with the object. The response that the individual makes depends upon the stimulus value or function that the object has for him in any particular situation.

Definition Of Absolute Pitch

The definition of the term "absolute pitch" has caused the greatest confusion in evaluating data from experimental work, because of the many different meanings of the term. As a result each investigator

criticizes every other investigator's work which yields results different from his own, by saying that the other investigator's subjects did not have absolute pitch or that the conditions allowed the subjects to use relative pitch, or any number of other similar reasons.

Petran (30) describes absolute pitch as follows:

> . . . on the psychological side is an inherent assumption of the extensity and telephone theories. With this assumption in mind, judgments of absolute pitch may be defined as judgments based on association learned between more or less narrowly limited ranges of the pitch series, . . . these judgments being without reference to or aid from any tone or tones recently heard which have been given as a standard or attended to in any degree as being of a certain pitch or familiar pitch position (30, p. 12).

Lewis (24) defines pitch as an attribute of auditory experience and not as a characteristic of acoustic waves.

> Pitch is that attribute of auditory experience which determines the positions of sounds in a psychological continuum extending from low to high . . . Pitch is no more a property of sound waves than color is a property of light waves. Pitch arises as an attribute of auditory experience when the ear is stimulated in appropriate ways, just as color arises as an attribute of visual experience when the eye is appropriately stimulated (24, p. 121).

The performance in absolute pitch is defined by most writers as designating a heard tone correctly from memory alone, without any other aid. In the opinion of the present writer, absolute pitch might be better defined as the ability to discriminate tones without the aid of other tones to such a degree that naming or pointing out the note is rarely incorrect.

Bachem (3) probably has made the most complete attempt to categorize ways of discriminating pitch. Genuine absolute pitch is based upon immediate recognition of the chroma, which would be the same definition most investigators would give to it. Quasi-absolute pitch, he says, is based upon aural or vocal standard and the interval sense. Pseudo-absolute pitch is based upon the estimation of tone height. Bachem's types of pitch would probably be agreed upon by most writers. He has only given names to the various degrees of pitch discrimination. However, it is only too obvious from the literature that many investigators have taken what Bachem calls quasi- or pseudo-pitch in judging genuine absolute pitch. For this reason a very reliable collection of data from absolute pitch experiments cannot be made.

Bachem says that the outstanding difference between the "absolute" pitch of most investigators and his type of genuine absolute pitch is the accuracy of judgment. The average error of judgment in cases of

"pseudo-absolute" pitch amounts to about five half-tones (after training) and in "genuine-absolute" pitch it is one-eleventh of a half-tone according to Abraham, and even less according to other authors. We would agree with Bachem and others that absolute pitch discrimination is a matter of accuracy of judgment. This evidence furthers all the more the explanation that absolute pitch is merely discriminatory ability developed to a very fine degree. Ability here does not imply some special gift which the individual has inherited, but a development of auditory discrimination to the greatest accuracy during the lifetime of the individual, based upon his reactional biography. Thus, absolute pitch is only the most accurate degree of pitch discrimination.

Although Seashore is probably the foremost proponent today of absolute pitch as a mysterious gift or power, he unknowingly substantiates our view by stating:

> As to the degree of accuracy of free tonal memory, we find a gradual transition from the very finest absolute pitch through all degrees of accuracy down to crude forms of acquired pitch. But it is convenient to make the distinction that absolute pitch may be thought of as involving accuracy to a very small fraction of a semitone; whereas acquired pitch is usually thought of in no finer terms than semitones (**37**, p. 18).

Theories Of Absolute Pitch

Practically all explanations of absolute pitch assume an inborn quality of some sort, and in some degree, although the trend in recent years has been to regard the ability as an acquisition. Probably the best known theories of absolute pitch are the so-called physiological theories. However, we shall not consider physiological theories in this paper for we strongly maintain that the stimulus object and situational factors involved must be considered, as well as the individual, in any explanation of behavior. The above does not mean that we are ignoring the important work that has been done by such men as Stevens (**40**, **41**, **42**), Thurlow (**44**), Wever (**48**), Wever and Bray (**49**, **50**, **51**), Wilkinson and Gray (**53**), and others upon differential "place" responses in the cochlea, but we maintain that even if such differential physiological reactions take place, they do not adequately explain pitch discrimination because they do not represent the entire behavioral event which occurs.

Physiological and physical data must not be confused with psychological behavior. Since we are interested in the psychological or behavioral event, that is, pitch discrimination, it is necessary for us to state what this psychological event consists of and thereby point out the futility of most of these theories, and their inability ever to give a

satisfactory explanation. In the act of discriminating a "pitch" we are interested in the particular individual determining a tone. That is, the tone is the stimulus and the factors that operate in the interaction between the individual and the tone are our problem. It is clear then that the theories based only upon physiological or physical data are not considering the actual psychological event, but rather only a part of it, the medium of contact. When we use the term, media of contact, we are speaking of the means by which the individual is able to react to the object. For example, in the case of pitch the individual is able to react to a tone because a medium of waves is present and they enable the individual to make contact with the sound of the stimulus object and to respond to it. The sound waves, then, are not the stimulus object but only the medium of contact between the individual and the stimulus object.

Practically all views of absolute pitch involve one of two explanations:

1. The individual may possess a special ability, differing from anything possessed by the average individual.
2. The individual may possess a high degree of some ability which is found to some extent in all persons.

Boggs (8), Révész (33), and Baird (5) are exponents of the first view. They believe that *quality* is the distinguishing feature of a tone in absolute pitch judgments. Boggs was at first inclined to believe that ability to hear overtones was the cause of keen pitch discrimination, but later changed her view from timbre to quality. Quality, she says, enables a person to hear tones in much the same way as those with color vision see colors. Baird's view is similar. He bases it chiefly on octave-errors and on immediateness of judgment, like the recognition of red when once it has been pointed out as red. Révész makes a definite distinction between those who recognize tones by pitch, and those who recognize them by quality. Both are supposed to have genuine absolute ear but the latter group has judgments more immediate and more correct. Révész holds that ability to distinguish quality is innate, like colors in vision, and cannot be acquired. Pitch-memory, on the other hand, may be acquired and perfected by diligent practice. To explain the influence of timbre, he says it is possible that when other parts of a tonal complex are changed the quality cannot be recognized in the new combination.

For von Kries (20), absolute pitch is something which cannot be acquired, while Stumpf (43) leaves the question open and gives more weight to familiarity, musical ability, feeling, and interest as displayed

in practical musical activities. Mull says, "The most convincing proof of the spuriousness of tonality, in Baird's sense of the term, would be success in training average individuals in absolute pitch to the point where they are similar to the 'gifted' individuals in every respect" (**28**, p. 475).

Seashore says, "Absolute pitch is an inborn predisposition which manifests itself during childhood and is strikingly immediate and spontaneous, . . . " (**35**, p. 18). He believes that absolute pitch involves errors of a small fraction of a semitone, whereas acquired pitch operates in terms of steps in the musical scale. Seashore considers that both are forms of memory and that both rest upon a favorable hereditary capacity, although both are influenced by training. But absolute pitch is influenced only to a slight extent, while acquired pitch can be learned to some degree by any person with a good sense of pitch and it is dependent upon knowledge of musical steps and notes.

Explanations of absolute pitch which do not rest upon a special ability are held by Meyer (**27**), Köhler (**18**), Watt (**45**), and Ogden (**29**).

Meyer (**27**) says that if we are to assume that some people are gifted with absolute pitch, it means that there is a physiological property, the lack of which prevents the acquiring of this mental faculty. It would be difficult to say what the physiological property would be. Rather than to make such an assumption, Meyer favors the idea that everyone potentially has the ability. He says that everyone has a certain amount of memory of pitch, since they are able to recognize and discriminate. Meyer also states that there is no reason proved by any experiment which shows that the individual by systematic and sufficiently lasting practice may not be trained to better "absolute pitch." He considers some features of the tone-body, rather than pitch, to be the basis of absolute pitch judgments.

The theory that tone-body, and not pitch, is important has been developed by Köhler (**18**). Tone-body includes brightness, vocality, volume, and intensity. It is recognized immediately without the intervention of a memory image. Watt (**45**) disagrees with Köhler's list of attributes and considers that Köhler has merely shown that timbre is important. Watt believes the attributes of tone to be two, pitch and volume. Both are taken into account by absolute ear, where there may be either a special refinement of sensory apparatus or a better auditory memory. But he thinks that in many cases of absolute pitch there is required also an absolute point of reference in auditory pitches. The problem is analogous to that of localization of the skin, where, however, the great influence of relativity does not exist. It is due to the lack of

relativity that tactual localization is possible for all persons. Auditory localization is possible only to a few who have not allowed the relative ear to dominate the absolute ear. According to this view, we all have had a chance to possess absolute pitch, but because of our education in musical relationships we have lost it through disuse.

Ogden (**29**) bases absolute ear upon a refined sensitivity to different clang-patterns and also insists upon the necessity of reference to a scale with which the individual who possesses the talent is familiar. This explanation is essentially like that of Watts. With Ogden, however, the point of reference seems to be introduced for purely systematic reasons and to be considerably overemphasized, especially when one remembers that good interval-judgment is not a requisite for absolute pitch.

Some investigators think that movement of the muscles in the throat are a help in making a judgment of pitch. Lotze (**26**) and Köhler (**18**) thought there must be a "larynx sensation " and thus pitch discrimination was limited to those with singing voices. Ladd and Woodworth (**22**) thought an innervation of the larynx is present when one is judging a note. Gough (**13**) in an extensive experiment could find no evidence of this. Wyatt makes this statement:

> . . . it seems probable that proficiency in a pitch discrimination test may be affected by many factors which are quite remote from simple auditory sensation and which may involve, not just the auditory sensorium, but possibly even the entire organism (**58**, p. 54).

Lewis has progressed farther than most investigators in attributing the pitch discrimination of an individual to that particular individual's experience. However, he is holding to traditional ideas for he says: "The sense of pitch, an innate capacity . . . "(**23**, p. 349). But then again, he also says: "Pitch is not a hard and fast thing like a physical quantity. To regard pitch as something rigid and fixed is to overlook the basic meaning of tonal experience" (**23**, p. 349). Lewis' main point is that pitch is not to be confused with frequency, which most of the older writers have made one and same (**30**).

Bachem (**4**) thinks inheritance, attention, and experience are the factors in the creation of absolute pitch. His factors present a more objective approach, especially if by inheritance is meant only the biological make-up of a human being. Attention is important for the individual must attend to or contact the stimulus object in order to interact with it, and experience is the reactional biography or life history that is more important in determining the individual's behavior (**16**).

Even though the interactional history with the tone stimulus may be subtle, it is clear that the reactions of the individual would have better opportunity to be highly developed if interaction with the tone were deliberately undertaken. We mean by this statement that the discrimination of pitch, even when it is developed to a point where it is "absolute," is developed during the lifetime of the individual. Absolute pitch is a matter of the keen development of attending to the stimulus (the tone) and the keen development of perceiving the stimulus (differentiating the tones).

If we are to consider that pitch discrimination is developed not from some inborn power but during the lifetime of the individual's reactions, it is not important whether the investigator elicits *pseudo, quasi, or genuine* absolute pitch in his experiment. It is not important because, from this interbehavioral view, the individual's accuracy of discrimination reflects only the degree to which he has interacted with tones during his lifetime. The individual with discrimination of the kind that we have chosen to call "genuine" absolute pitch has obtained a psychological interaction with different tones equivalent to what most of us have with the various colors. This does not mean necessarily that the individual has to "train" himself or to practice deliberately with tones in order to judge them. The interaction with the tone stimulus could be as subtle as any psychological interaction can be; for example, as subtle as how we learn to distinguish red from green.

That there are usually great differences between those who "have" absolute pitch and those who are "trained" along that line is not the crucial point which most writers seem to think. The point that needs to be emphasized is the uselessness of merely attributing absolute pitch to some unknown power. If we are to find a scientific explanation, we must deal with the actual event of the tone and the organism discriminating it. With an interbehavioral explanation we can deal with the facts as they are present in the crude event (16), viz: the discriminating of pitch so accurately that it is "absolute" and the learning of this ability during the lifetime of the individual. This means that we will have to consider the individual's reaction from the beginning of his psychological interaction with tones of any sort. How the person will develop his reactions to tones depends upon the individual in his life conditions. Therefore, the individual who cannot discriminate pitch after training as well as the individual with absolute pitch but without special training is not able to overcome the effects of reaction to tones that developed during his lifetime before training was started. We know that the first years mark the beginning of basic permanent behav-

ior, and so the older the person the more difficult it becomes for him to be influenced by training (**16**).

Investigations Of Absolute Pitch

Some main points in the results of experiments that Petran (**30**) reviewed up to 1932 were that white notes on the piano are more often correctly judged than black; that notes in the middle range are judged more accurately than those in the extreme range; and that there is a tendency to judge high notes lower and low notes higher than they actually are. In a recent experiment Riker (**35**) found that subjects with musical experience were able to judge pitch better in the middle range. All of these results show better judgment with notes that in general are used most often. In other words, notes that are played more often and experienced more often by the individual are the ones he knows best.

Most investigators did attempt to consider experience as an explanation, for one of the most frequent explanations offered was that the more familiar a timbre, the easier it was to judge. However, von Kries (**20**) and Abraham (**2**) both rejected this explanation. Stumpf (**43**) and Köhler (**18**) both concluded that experience must be considered. Chiloff (**10**) concluded that absolute pitch depends on the faculty of determining the timbre of sounds of different heights. Baird (**5**) found subjects trained in music to be less accurate with tones on other instruments than on a piano. Köhler (**18**) holds that unfamiliar timbre recognition is quite impossible. Gough (**13**) after training both musical and unmusical subjects found only comparatively slight differences in accuracy of judgment with different timbres. Thus, from these results it seems that familiarity of timbre is important in judgment of pitch. Further results pointing to familiarity are the experiments by Stumpf (**43**) who found that a bass player was better in judging tones in the lower range than in the rest of the scale and that a violinist was better in judgment in the upper range.

Association with tones. Association with tones was reported by many observers such as Stumpf (**43**), Whipple (**52**), Boggs (**8**), and Gough (**13**). One was a keyboard association so that the subject had a spatial position of the note. Another was an association with notes of a prominent song or triad built on the tone. A non-musical association was with pain, such as with high notes, or tones sounding woody or muffled to the subject. All these results suggest that past experience is necessary before the associations with which to make judgments can occur.

Practice effects. Practically all of those individuals with absolute

pitch are musicians of more than average competency (**28**). Bartholomew (**6**) found that proficiency of subjects increased with the number of ear training and harmony courses they had. Jadassohn before 1899 stated that "the faculty of absolute pitch was easily acquired" (**15**, p. 33). The first experiment on acquiring absolute pitch was made by Meyer (**27**) on himself and another person. They learned to name tuning forks from 100 to 4500 vibrations per minute by their vibration numbers, and thirty-nine notes on the piano, with a rather high degree of accuracy. They began with a few pitches at a time and added new ones from time to time. They did this with tuning forks and a piano. With a choice of 39 pitches, more than half of their judgments were correct. After several years the subjects lost the greater part of what they learned for want of continued practice. Boggs (**8**), experimenting on herself, improved her judgment by paying special attention to overtones; and Köhler (**17**) did the same thing by concentrating on what he calls "body-tones."

Both Gough (**13**) and Mull (**27**), experimenting with college students, found that a certain amount of pitch naming can be acquired by musical and unmusical subjects. The two experiments show that systematic practice in pitch identification is effective.

The results of Mull's experiment (**28**) show that ability to judge notes correctly can be greatly improved by training. The improvement is largely immediate and is relatively lasting in its effects. Mull states that in making judgment of absolute pitch a high degree of attention to the notes is more effective than a lower degree, and a note which is not attended to has no noticeable effect. She also finds that the ability to acquire absolute pitch correlates better with practical musical ability than with ability in the Seashore tests. Mull concludes that the average person can acquire absolute pitch. She bases this statement on the fact that there is a similarity of performance between Baird's "gifted" subjects and Mull's group of average persons after extensive training. Mull goes on to state that the possession of absolute ear seems to rest upon an interest in the notes themselves rather than in their melodic or harmonic relationships. The importance of attention is indicated by (a) the immediateness and permanence of the learning, (b) the individual variability from day to day, (c) the fact that a stimulus which is not attended to has no noticeable effect, and (d) the fact that a good hearing of the note is necessary before judgment can be made.

But, as Mull says, "There still remains the question why certain individuals without having been trained possess absolute ear so much more completely than others." Mull's answer to this question presents

one possibility for the development of absolute pitch and is certainly more feasible than attributing it to an inborn quality. She says: "An answer may lie in the suggestion that such persons, probably when they were children, found notes interesting in themselves . . . " (**28**, p. 492).

The percentage of correctness among "gifted" persons in Baird's study (**7**) ranged from 100 per cent to 0 per cent, both by group and by individuals. Boggs (**8**) percentage of correct judgments in his "gifted" subjects ranged from 100 per cent to 16 per cent in the same individual. Mull (**28**) thinks it depends upon experimental conditions and upon the individual himself.

Most investigators found the middle pitches to yield the greatest precision in judgment, but this was not always the case. Stumpf (**43**) recognized that the greatest precision is in the middle region and that pitch discrimination is finest there. But he attributes it to greater use and consequent familiarity, for we have already mentioned his subjects, the bass player and the violinist, who discriminated best at the lower and higher ranges, respectively.

Riemann (**34**) said that some musicians have absolute pitch because of their frequent tuning of their instruments. Wilson (**55**) agrees with this. Wyatt (**58**), in one of the most recent experiments, showed that pitch discrimination of initially "pitch deficient" adults was significantly improved after intensive training. She places special emphasis on the need for the right kind of training in order to get improvement in pitch discrimination.

Another argument for the sufficiency of an explanation on the basis of previous experience and familiarity is the fact that many investigators have found some order of difficulty among various instruments. Von Kries (**20**) finds general agreement among musicians that the order from least to most difficult is: piano, strings, wind instruments, voice, whistling, tuning forks, and bells. Baird's order (**18**) is piano, organ, voice, and tuning forks. Mull (**28**) points out that it is possible that this could be due to familiarity, but Baird (**7**) and von Kries (**20**) do not care to believe it. The fact that Baird and von Kries have a slightly different order shows that it depends upon the particular individual's experience as to what order he would choose. A piano player might be more likely to judge tones correctly on any musical instrument rather than on a tuning fork or bells with which he would have little contact. More specifically, a violinist may be able to judge tones more accurately than a pianist because it is necessary for him to play tones on his instrument by placing his finger in the correct position; but a pianist has

merely to touch a key which hits a string already tuned to a particular tone. Bachem (4) substantiates this by his statements:

> Musical experience, however, plays a role in the acuity of absolute pitch. . . . Several (subjects) believed that musical experience improved their quality of absolute pitch, and lack of experience allowed it to decline. . . . Two musicians found their absolute pitch declining with increasing deafness, which had not progressed sufficiently, however, to interfere with their musical activities. . . . A somewhat greater acuity of absolute pitch was found in violinists than in piano players (4, p. 439).

Cameron (9) conducted an experiment to see the effects of practice in discrimination and singing of tones. He found no marked correlation between the initial capacities of the subjects tested for discrimination of tones and ability to reproduce these tones accurately by singing. Practice in singing tones of a certain pitch resulted in marked reduction in the error of reproducing those tones in the case of four of six subjects. Slight improvement in singing tones of a pitch different from the one practiced was made by three subjects and no improvement in the other. Subjects who improved in accuracy of singing tones of a certain pitch improved also in discrimination of tones at that level. There was no improvement for subjects in the discrimination of tones of a different pitch from that practiced. Subjects who did not improve in accuracy of singing made no improvement in discrimination.

Lewis (25) conducted an experiment to secure data for constructing and comparing pitch scales. The scales he constructed did not agree with each other nor with scales based on the findings of Shower and Biddulph (39) and of Stevens and Volkmann (42). The difference could not be attributed to experimental error. Lewis concludes that any pitch scale depends, in some measure, upon the group of persons who made the required judgments and upon the particular method of observation employed.

If it is true that pitch depends upon the particular individual's judgment, then each judgment would not necessarily be alike, as Lewis' investigation shows. This result certainly is not evidence that pitch is an inborn quality, for if so, then a certain number of vibrations of a tone would be heard as exactly the same pitch judgment by every individual. Rather, it is evidence that the ability to discriminate pitch is acquired in the person's lifetime, and that accuracy in hearing a pitch as a certain note depends upon his own reactional biography.

Bachem (4), in one of the most recent experiments, studied 103 cases from which he devised his three types of absolute pitch discrimination:

genuine, *quasi*, and *pseudo*. All of these types of absolute pitch discrimination occur with variation in acuity, range of the musical scale, and influence of timbre. These variations occur because of differences in past experience and in familiarity with tones.

Bachem in an investigation to secure comparative results conducted identical experiments on persons with and without absolute pitch. A first pair of experiments was performed on the piano by offering a tone to a person placed back toward the piano and then having the person turn toward the piano and identify the key without striking it. Twenty tones were judged this way and another twenty were identified by striking the key. In order to compare the results of this with accuracy of absolute pitch judgment, several ranges of the piano scale were tuned to eight tones, and marked correspondingly. Most subjects identified the tones correctly.

A second pair of experiments was designed for more accurate results. A tone was offered several times on a tone variator which was standardized in vibrations per second. Every five minutes a different tone was sounded and altered according to directions of the subject, until the tone was considered the right one. A similar experiment was conducted with the five-minute intervals replaced by half-day intervals.

The average error of absolute pitch determination was 0.6 per cent for observations extending over one-half hour. For persons without absolute pitch, the error amounted to about 8 per cent on the tone variator, representing an interval of one to two half-tones. On the piano an error of about five half-tones was found. With these results Bachem says there is conclusive evidence of the great difference in accuracy of pitch identification in persons without and with absolute pitch.

As Bachem explains, attention, or what the interbehaviorist would call attending to stimuli (16), is an important factor to be considered. Attending to stimuli we must consider as having the same sort of chance for development as reacting to tones, given in the explanation above. An obvious example is the development of hearing of a blind person. Bachem (4) tells of a blind person without musical training who detected his absolute pitch accidentally during a conversation with a possessor of absolute pitch and he was able immediately to identify tones correctly after he knew how to denote them. Bachem admits that the large percentage and high degree of absolute pitch in congenitally blind persons shows that inheritance is not the only factor. Eleven of his 103 cases were blind since birth and in none of these cases were there any relatives who had absolute pitch. Bachem also admits that there is an influence of forced attention to sound by the blind.

Regardless of such evidence, Bachem shows that he is influenced by

traditional views for he thinks that heredity plays some part. "An alternate possibility," he says, "could be that the musical environment surrounding these families played an important role" (**4**, p. 437). He rules this out in the case of two families that he studied, because in one the grandfather had absolute pitch and the mother was not very musical. In another, two musical prodigies studied on a piano which was so much out of tune that they had to distinguish between the name of the key and the actual tone produced. Considering the first case, there is no indication that the child had not associated with the grandfather and also according to an interbehavioral explanation, it would not make any difference if the child had not associated with anyone who had absolute pitch. In regard to the second case, the conditions under which the children worked would mean that they would have to attend to tones even more carefully than most students, and this would help, not hinder, the acquisition of absolute pitch.

It is true, of course, that a musical environment may help the development of pitch discrimination because of the added opportunity to attend to tones. Absolute pitch occurs at a decidedly higher percentage in musical groups, particularly professional musicians. Bachem states, "Especially is this true of those with early musical training." He goes on to say, "It seems that attention to musical tones in early youth plays a predominant role in the development of absolute pitch" (**4**, p. 439).

Besides the statements made by these investigators, the results of many other experiments show the incorrectness of saying that absolute pitch is an absolute inborn quality. Meyer and Heyfelder (**27**) attempted to train themselves and their results show a high degree of correct judgments. Köhler (**18**) after two weeks of practice could make 112 correct judgments out of 220. Gough (**13**) trained 90 students and got an improvement. Mull's data (**28**) show that practice increases the percentage of correct judgments from 40 per cent to 82 per cent. Wedell states, "It is evident that some of the later experimenters are aware that even in absolute pitch, the judgment is at least in part one of relation" (**46**, p. 488).

Wedell's experiment led to these conclusions:

1. Relatively unmusical observers can increase their accuracy.
2. Greatest increase is in the first few practice sessions.
3. Limit of ability is an average error of about three semitones.
4. The course of the learning process is very irregular and there are large individual differences.
5. Unmusical subjects can learn accurately and easily to recognize tones that are eight and one-half semitones, but they fail to judge intervals when decreased to five and one-half semitones or less.

Riker (35), in an experiment comparing subjects who said they had pitch-judging ability with those who disclaimed it, concluded that ability to judge pitch was not confined to specially trained or talented subjects. He also concluded that there were various degrees of accuracy in judging pitch and that it was "... a function of day to day experience with music" (35, p. 346).

Absolute pitch in children. Very little study has been made on children by early investigators, but Abraham (1) trained three four-year-old girls to sing "Ade" to the mode a-d. One had been to kindergarten where songs were transposed and she sang it at various pitches but with the correct interval; the other two were always correct even after three months and one after one and three-quarter years.

Bennedik (7) had three children who all acquired absolute pitch ability but in different measures. Copp (11) taught children to name heard tones. From her experience with school children she states that 80 per cent of all children can acquire absolute pitch. Katz (17) and Bennedik (7) substantiate this but not with as high a percentage. Komatsu (19) experimented with a child, four years and ten months at the beginning of training, who was able, after one year, to discriminate 46 harmonics consisting of three or four tones.

A number of experiments as to whether absolute pitch can be acquired by anyone with normal hearing who wants to learn have been performed using tuning forks, piano, and oscillators (34). All experiments point to the conclusion that pitch naming ability can be improved to a certain extent by training. Since the learning period seldom covered more than a few months no greater result could be expected.

Probably the most significant and thorough experiment was conducted by Wolner and Pyle (56) to show what effect age and practice have upon pitch discrimination. Music teachers in three elementary schools selected pupils showing the greatest deficiency of pitch discrimination. From this group the seven poorest were selected. No pupil selected could distinguish the thirty-vibration differences on the forks. On the piano, in general, they could not distinguish differences of the octave, fifth, third, wholetone, or semitone. None of them could sing, although they had been in music classes from the first grade and were at the time of the experiment in the fifth, sixth, and seventh grades. There were three boys and four girls.

Each pupil received individual instruction and tests for twenty minutes each morning, five days a week. The whole number of hours spent in training was on the average 16 for each pupil, extended over a period of 81 days.

The definition and meaning of pitch, or "high" and "low," as distinguished from intensity, duration and timbre were strongly and repeatedly emphasized, particularly in the early days of training. The pupils were led to see the necessity of thinking of tones as one would think of a problem. Interest, attention, and concentration were worked for in the method of teaching.

All seven pupils learned to discriminate perfectly the intervals of octaves, fifths, wholetones, and semitones in the range from A, sixteen tones below middle C, through A, thirty-four tones above middle C, a tonal range of four octaves. There was great variability in the time required to reach a given degree of efficiency, and in the response to methods and changes of methods. With the forks, four of the pupils became perfect in distinguishing all the pitch differences from the largest, thirty vibrations, down to the smallest difference, one-half a vibration. Of the other three pupils, one learned to distinguish perfectly down to two vibrations difference, another down to three vibrations difference, and the third down to five vibrations difference. The standard of perfection was 10 out of 10 trials correct.

Each pupil improved noticeably in ability to sing. At the conclusion of the experiment, one pupil sang the words and music of several songs, with no trace of pitch deficiency; and also sang major and minor scales, chromatics, intervals, and tones picked at random. Another sang scales and intervals and the music of a song without words. Two pupils sang scales and intervals. The other three pupils sang, not perfectly, but with tremendous improvement over their initial efforts.

Conclusions

The statements made by the writers we have quoted and the results of the many experiments we have discussed certainly show the need for an adequate explanation of absolute pitch. The results are:

1. Absolute pitch and lesser degrees of pitch discrimination can be acquired by some individuals. Thus, it proves false the idea that absolute pitch is some rare power over which we have no control.
2. Behavior that closely entails pitch discrimination, such as musical experience and musical training, allows a much better opportunity to develop keener pitch discrimination.
3. Pitch discrimination can be acquired readily in early age, which may be an indication that the younger the reactional biography the easier it is to acquire new reactions.
4. The development of keener attending to stimuli makes for a keener development of pitch discrimination. Obvious examples of this are congenital blind persons and child prodigies.
5. The sublety of behavioral development is brought out by those indivi-

duals who have built up in their lifetime keen pitch discrimination and suddenly realize that they are able to perform such behavior.

We can see that an interbehavioral view is justified by the results of past experiments. Basically, the interbehavioral explanation of absolute pitch rejects the conception of an inherent faculty or quality. It accounts for absolute pitch and all pitch discrimination as behavior developed within the lifetime of the individual. This means that the discrimination is due to the way in which the individual builds up reactions to sounds, and more specifically to the thoroughness in his development of attending to tones (stimuli). Accordingly, the individual's reactional biography, built up from past experience, is the important factor in determining what sort of tonal discriminations he will make. It follows then that "absolute pitch" is nothing more than a fine degree of accuracy of pitch discrimination. The pitch of a tone is the sound that the particular individual learns as that particular tone.

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SELECTIVE SAMPLING IN PSYCHOLOGICAL RESEARCH

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Through a long process of indoctrination, psychologists have been made fully aware of the importance of statistical tests of significance in the analysis of data. They are, unfortunately, less aware of some of the basic requirements underlying these tests.

Some thought has been given by research workers in psychology to the importance of the form of distribution in the population sampled. Much less attention has been given to the even more important question of the method used in drawing a sample. This article presents two examples of difficulties in the interpretation of data arising from methods of sample selection fairly common in psychological research (and in research in other fields).

This article is not intended as a criticism of individuals. Residents of glass houses should avoid rock-heaving, and the writer's own research work has suffered notably from the very defects stressed in the present analysis. The writer cannot even promise not to repeat past errors since practical considerations frequently defeat the best of intentions. It is important, however, that all of us realize the limitations of our techniques and not claim for our conclusions a validity not warranted by our methods.

The theoretical consequences of selective sampling are well established and have been cited frequently. Nevertheless, this theory has been ignored fairly consistently in psychological research. All of us have been, to a greater or lesser extent, responsible for this situation. The examples selected for this article are not unique nor even unusual. The two sets of data analysed herein were selected partly from considerations of ease in analysis and partly because they are drawn from studies which were carefully planned and executed. The writer feels that the results of these two studies are generally valid but that it is necessary to point out that this validity cannot be conclusively established by the sampling techniques used.

A fundamental principle of sampling is that random selection must be present at some point in the sampling process. Without random selection it is not possible to determine whether the results obtained represent basic characteristics of the population or whether they are by-products of the sampling process. This fundamental consideration has not been sufficiently stressed in psychological methodology, al-

though many psychologists are conscious of the difficulty and troubled by it at a "semi-intuitive" level.

A large number of psychological studies use groups drawn from such selected sub-populations as college students, hospital inmates, nursery school children, etc. While the use of such sub-populations is perfectly proper, studies based on these groups have frequently been used to draw conclusions not restricted to the sub-population sampled. Such conclusions can be considered valid only when we know the facts regarding the selection of individuals present in the sub-population.

In the June 1946 *Biometrics Bulletin*, Berkson (4) discusses the limitations of fourfold table analysis with respect to data on the relationship of diabetes and cholecystitis in a hospital population. He assumes a general population in which the two conditions (diabetes and cholecystitis) are uncorrelated and points out that, if we assign to each condition a probability of a patient having the condition coming to the hospital for treatment, the hospital population of these two characteristics may show a correlation even though the two selective rates operate independently. Such a spurious correlation will exist in the hospital population unless the two selective rates are equal or one of the selective rates is zero (i.e. the probability of coming to the hospital is not affected by one of the conditions). Berkson notes:

> Except for such cases there does not appear to be any ready way of correcting the spurious correlation existing in the hospital population by any device that does not involve the acquisition of data which would themselves answer the primary question. For instance the device sometimes used of setting up in the hospital sample a one-to-one control so that both groups examined have the same number of cases and are identical as regards say, age and sex, does not touch the difficulties referred to here.

Berkson's comments have much wider applications than hospital populations and fourfold tables. The problem he describes will exist with regard to any type of correlational analysis applied to any selected sub-population. The likelihood of selective bias is increased if the selective processes which generate the sub-population do not operate independently. Berkson points out:

> The assumption that these probabilities operated independently in an individual who is suffering from more than one disease is doubtless oversimple. In general we may guess that, if a patient is suffering from two diseases, each disease is itself aggravated in its symptoms and more likely to be noted by the patient. So far as this difference of fact from assumption goes, its effect would be to increase relatively the representation of multiple diagnoses in the hospital, and in general to increase the discrepancy between hospital and parent population even more than if the probabilities were independent.

In a great many cases sub-populations arise from compound rather

than independent selective factors. College student groups are a selected population with reference to both intelligence and economic status and the selection is undoubtedly not independent for the two factors. Assume that a given college admits only U. S. born males in the age range 17 to 23 years, and therefore, that the population from which the college students are drawn may be defined as all native-born U.S. males aged 17 to 23 inclusive. Suppose further that the college admits 3% of those individuals in the population having I. Q.'s of 120 or more and 2% of those individuals coming from families with annual incomes of $15,000 or more. If the admission process operated independently with respect to the two factors, we should expect that the college would admit 4.94% of the group of individuals having both an I. Q. of 120 or more and a family income of $15,000 per year or more (i.e. 3% on the basis of I.Q. only, plus 2% of the remaining 97% on the basis of family income only). Actually the two factors may reinforce each other so that a larger number of individuals with both high I.Q. and high family income would apply for and be admitted to the college than would be expected from the consideration of the separate probabilities.

There are presented below selected results from two recently published research studies. The material presented does not represent the whole of either study. The present analysis covers only a small part of the conclusions of each study. We are concerned only with the particular phases of the studies presented herein and not with the validity of the studies as a whole. However, there are no facts presented in the complete published reports which would require modification of the analysis presented herein.

Study I: Allport and Kramer, "Some Roots of Prejudice"

Allport and Kramer (1) studied prejudices of 437 college undergraduates enrolled in elementary courses in psychology at Dartmouth, Harvard and Radcliffe. They note:

> While there is reason to suppose that college students (perhaps especially those studying psychology) are disposed to give less prejudiced responses than a more unselected population, still there is no ground for believing that their life-experiences with which the questionnaire deals, are in any significant way atypical.

As will be seen from the analysis presented below, the selection of the group studied by Allport and Kramer may very well influence the results obtained.

Allport and Kramer measured prejudice by use of a set of statements concerning Negroes, Jews and Catholics. These were scored on a 4-point scale in such manner that a high score indicated most prejudice

TABLE I

POPULATION AND SELECTION FACTORS WHICH COULD PRODUCE RESULTS SHOWN BY ALLPORT AND KRAMER

	*Hypothesis I**		*Hypothesis II*		*Hypothesis III*	
	Less Prejudiced	*More Prejudiced*	*Less Prejudiced*	*More Prejudiced*	*Less Prejudiced*	*More Prejudiced*
A. *Population Number in Each Group*						
Both parents college graduates	.65A	.35A	.40A	.60A	.40A	.60A
One parent college graduate	.65B	.35B	.40B	.60B	.50B	.50B
Neither parent college graduate	.65C	.35C	.40C	.60C	.65C	.35C
B. *Proportion of Population in Sample*						
Both parents college graduates	.03059	.03737	.04000	.01756	.04000	.01756
One parent college graduate	.01074	.01768	.02000	.01182	.01500	.01330
Neither parent college graduate	.00424	.01123	.01000	.00952	.00424	.01123
C. *Number in Sample*						
Both parents college graduates	.01988A	.01308A	.01600A	.01054A	.01600A	.01054A
One parent college graduate	.00698B	.00619B	.00800B	.00709B	.00750B	.00665B
Neither parent college graduate	.00275C	.00393C	.00400C	.00571C	.00275C	.00393C
D. *Sample Distribution*						
Both parents college graduates	60.3%	39.7%	60.3%	39.7%	60.3%	39.7%
One parent college graduate	53.0%	47.0%	53.0%	47.0%	53.0%	47.0%
Neither parent college graduate	41.2%	58.8%	41.2%	58.8%	41.2%	58.8%

* This hypothesis assumes independent selection rates of:

Less prejudiced	.0030
More prejudiced	.0100
Both parents college graduates	.0276
One parent college graduate	.0078
Neither parent college graduate	.0012

and a low score least prejudice. Table 21 of the report presents the following distribution:

PERCENTAGE DISTRIBUTION OF PREJUDICE SCORES AS A FUNCTION OF PARENTAL EDUCATION

	Less Prejudiced	*More Prejudiced*
Both parents college graduates	60.3	39.7
One parent college graduate	53.0	47.0
Neither parent college graduate	41.2	58.8

In the above table "less prejudiced" denotes those subjects with scores below the median for the entire group of 437 undergraduates and "more prejudiced" denotes subjects with scores above the median. Allport and Kramer conclude from this table that

> . . . students' prejudice scores bear an appreciable relation to the educational level of their parents. This finding is in line with the knowledge we now have that higher educational achievement on the part of an individual tends to be associated with fewer hostile attitudes. The novelty of the present finding consists in the demonstration that the favorable association between educational level and freedom from prejudice reaches back into the preceding generation: the more educated the *parent* the more free is the *child* from prejudice.

While the above finding may be quite reasonable, it is not the only interpretation possible of Allport and Kramer's results. Table I shows (as Part A) three hypothetical populations from which Allport and Kramer's sample might have been drawn. Selection ratios shown in Part B of Table I would yield from the hypothetical populations a distribution in the sample identical with that presented by Allport and Kramer in their Table 21.

Allport and Kramer's sample could have been drawn (as they assume implicitly) from a general population in which children of college graduates are less prejudiced than children of parents who are not college graduates. However, the sample could have come from populations with no difference in the degree of prejudice among the three groups (as in Hypotheses I and II of Table I) or from a population in which children of college graduates were more prejudiced than children of parents who are not college graduates (as in Hypothesis III of Table I).

No one can demonstrate that any one of the three hypotheses presented in Table I is correct or that the hypothesis that Allport and Kramer's group represented an unselected sampling of the general population is more or less likely than any of the three hypotheses presented in Table I. It is evident that no final conclusions are possible in the absence of information regarding the actual selection rates of the sample.

It should be noted that the three hypotheses presented in Table I are not unique. In the absence of knowledge about the actual selection rates, there are an infinite number of sets of possible populations and selection rates which could yield the results obtained by Allport and Kramer.

STUDY II: BERDIE, "RANGE OF INTEREST AND PSYCHOPATHOLOGIES"

Berdie (2) has developed a list of items to be used in measuring range of interests. Subjects check on a list of 22 activities, those which they "like to do." The 22 items include such activities as playing checkers, dancing, going on dates, hiking, reading, playing baseball, watching baseball, etc. Berdie administered this check list (orally to some groups and in printed form to others) to recruits at a U.S. Marine Corps recruit training station. He found that recruits recommended for inaptitude discharges (by the psychiatric unit) checked significantly fewer interests than did "normal" recruits.

In a second study (3), Berdie reports on range of interests for a group of 228 marine recruits recommended for inaptitude discharges. Berdie notes that the "abnormal" recruits contained "a heterogeneous collection of diagnoses, including various types of neuroses, constitutional states and organic conditions." For each diagnostic group, Berdie presents separate mean interest scores.

Questions can, of course, be raised regarding the adequacy of diagnoses and the meaning of the diagnostic categories used. This fact, in itself, operates as a "selective factor." Berdie states that the number of cases in each diagnostic group "can not be interpreted as showing how frequently these diagnoses occur in the Marine Corps or Naval Service, as no attempt was made to obtain a representative sample." It is obvious, of course, that even a representative sample of Marine Corps recruits with a given psychiatric diagnosis is not necessarily a representative sample of all individuals in the population having the given condition nor even of all individuals in the Marine Corps in a given psychiatric category (since undetected conditions must obviously have existed).

Berdie's group included 13 men classified as "epileptic" who showed a mean interest score of 10.23 items checked and 20 men classified as "constitutional psychopathic state-inadequate personality" who showed a mean interest score of 5.80. These two groups had respectively the highest and lowest mean interest scores of the 10 diagnostic categories reported in the study. Berdie states that "the probability of this difference" lies between the .01 and .02 levels.

In Table II are presented two hypothetical populations and sets of

TABLE II

POPULATIONS AND SELECTIVE FACTORS WHICH COULD PRODUCE RESULTS SHOWN BY BERDIE

		A. HYPOTHESIS I			
		CPS-Inadequate Personality		*Epilepsy*	
Interest Score	*Population Distribution*	*Selection Rates*	*Distribution in Sample*	*Selection Rates*	*Distribution in Sample*
0	.03479	.002868	.0998	.001185	.0412
1	.03715	.002624	.0975	.001179	.0438
2	.03930	.002392	.0940	.001171	.0460
3	.04122	.002172	.0895	.001159	.0478
4	.04291	.001960	.0841	.001145	.0491
5	.04438	.001760	.0781	.001129	.0501
6	.04563	.001568	.0716	.001110	.0507
7	.04663	.001392	.0649	.001090	.0508
8	.04743	.001224	.0580	.001069	.0507
9	.04800	.001064	.0511	.001046	.0502
10	.04834	.000920	.0445	.001023	.0495
11	.04844	.000784	.0380	.001000	.0484
12	.04834	.000660	.0319	.000977	.0472
13	.04800	.000544	.0261	.000954	.0458
14	.04743	.000444	.0210	.000931	.0442
15	.04663	.000348	.0162	.000910	.0424
16	.04563	.000268	.0122	.000890	.0406
17	.04438	.000200	.0089	.000872	.0387
18	.04291	.000140	.0060	.000855	.0367
19	.04122	.000088	.0036	.000841	.0347
20	.03930	.000052	.0020	.000829	.0326
21	.03715	.000024	.0009	.000821	.0305
22	.03479	.000008	.0003	.000815	.0284
Total	1.00		1.00		1.00
Mean	11.00		5.80		10.23
Variance	40.00		20.65		39.41
S. D.	6.32		4.54		6.28
		B. HYPOTHESIS II			
0	.02	.002340	.0468	.000376	.0075
1	.03	.002121	.0636	.000383	.0115
2	.04	.001912	.0765	.000407	.0163
3	.05	.001715	.0858	.000446	.0223
4	.07	.001529	.1070	.000500	.0350
5	.08	.001355	.1084	.000571	.0457
6	.09	.001191	.1072	.000656	.0590
7	.10	.001039	.1039	.000758	.0758
8	.10	.000898	.0898	.000875	.0875
9	.09	.000769	.0692	.001007	.0906
10	.08	.000650	.0520	.001156	.0925
11	.06	.000543	.0326	.001319	.0791
12	.05	.000448	.0224	.001499	.0750
13	.04	.000363	.0145	.001694	.0678
14	.03	.000290	.0087	.001905	.0572
15	.02	.000228	.0046	.002131	.0426
16	.02	.000177	.0035	.002373	.0475
17	.01	.000138	.0014	.002630	.0263
18	.01	.000110	.0011	.002903	.0290
19	.01	.000093	.0009	.003192	.0319
Total	1.00		1.00		1.00
Mean	7.95		5.80		10.23
Variance	16.85		11.78		18.64
S. D.	4.10		3.43		4.32

selective sampling rates for individuals diagnosed as epileptic and as "CPS-inadequate personality." In both hypotheses it is assumed that the general populations for both psychiatric groups have identical interest score distributions but that a difference in selection rates for the sample exists. Such differences might arise from any of the selective factors noted above (i.e. selection from the general population for Marine Corps service, selection from Marine Corps inaptitude dischargees for inclusion in the sample or faulty diagnosis). It might also arise from an association between range of interests and likelihood of detection of the condition, e.g. "constitutional psychopaths with inadequate personality" with a small range of interests might be more apt to come to psychiatric attention than those with a large interest range.

Whatever the true situation may be, we cannot conclude that differential sampling did not occur without specific data to demonstrate this fact. We cannot even assume that random selection was just as likely in this situation as differential selection. Such an assumption is the equivalent of the old fallacy of assuming that two events are equally probable in the absence of information regarding the actual probabilities (and then proceeding to demonstrate that there is almost certainly mammalian life on Mars).

From Table II it can be seen that the differences in mean score obtained by Berdie could have arisen through selective sampling. The result is not confined to Berdie's data. No confidence can be placed in obtained differences (regardless of "measures of significance") unless the experimenter has controlled carefully the method of sample selection and knows the probabilities of selecting a given element. If we have such knowledge, we can make proper estimates of population parameters and attach to our statistics properly determined measures of sampling error. Without such knowledge, generalization from sample to population is extremely risky. Since the application of a test of significance implies generalization from sample to population, all of our most valuable statistical techniques are meaningless without proper control of the *sampling method.*

It should be noted that a result which applies to sample means applies equally well to coefficients of correlation, analyses of variance, factor analysis, etc., since these techniques go back basically to differences in group means. It should be noted also that selective sampling affects the variance within groups as well as the variance between groups. In Table II the variance within the groups sampled is less than the population variance in three cases and greater in the fourth. The effect of selective sampling on total variance can only be determined

from a knowledge of the number in each group and the selective rates for each group. Thus, measures of correlation may be affected by selective sampling by distortion of both variance within groups and variance between groups.

Summary

Through the use of two published psychological research studies, the possible effects of selective sampling are emphasized. The present analysis stresses the importance of control of the sampling process if valid generalizations are to be drawn from research data.

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ADDITIONAL TABLES OF THE PROBABILITY OF "RUNS" OF CORRECT RESPONSES IN LEARNING AND PROBLEM-SOLVING

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In a previous paper (2), the writer demonstrated how "runs" of "correct" responses and grouping of "correct" and "incorrect" responses could be used as criteria of learning or problem-solution in experiments involving repeated trials.[1] The present paper has a double purpose: (a) to extend the tables of the probabilities of "runs" given in the earlier article; and (b) to discuss an important aspect of repeated statistical tests.

As pointed out in the original paper, a "run" of correct responses in a properly controlled learning or problem-solving experiment may permit the experimenter to conclude (at a specified confidence level, ϵ) that his subject has learned or solved the problem. A brief table of probabilities of chance runs was published in the original paper. Probabilities were given for various length runs of correct responses in total sequences of 16, 20, 25, and 50 trials, where the probability of a chance success on a single trial was 0.5. Because of the usefulness of the runs criteria, the limited table of the previous article has been extended in Table I of the present paper. In Table I, the probabilities of runs are given for total sequences of 16, 20, 25, 30, 35, 40, 45, and 50 trials where the probability of a chance success on a single trial is $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, or $\frac{1}{5}$.[2]

Two obvious trends will be noted in the probability entries of Table I, and both of these trends have direct implications applicable to the effective use of the runs criteria. First, the probability of any given run of correct responses decreases markedly as the probability (p) of a chance success on a single trial becomes small. Second, with any specified p, the probability of any given run of correct responses increases with an increase in the total number of trials in the series.

[1] Child (1) has subsequently pointed out an error in the original paper and showed how Yates' correction for continuity would, under certain conditions, improve the normal approximation formulae for the binomial.

[2] The entries of Tables I and II were computed for the writer by Marie H. Glissendorf, working under the supervision of Professor K. J. Arnold of the Mathematics Department, Director of the Computing Service of the University of Wisconsin. Each probability was computed from one or more of the formulae given by Uspensky (3, Ch. 3). Entries in Table I correct some errors in the fourth decimal place which occurred in Table I of the earlier paper (2, p. 278).

TABLE I

THE PROBABILITY OF A RUN OF AT LEAST S SUCCESSES IN N TRIALS WHEN THE PROBABILITY OF A SUCCESS IN A SINGLE TRIAL IS p.

S \ N	16	20	25	30	35	40	45	50
				p=1/2				
5	.1965							
6	.0929	.1223	.1578	.1918				
7	.0429	.0582	.0770	.0953	.1134	.1310	.1484	.1653
8	.0195	.0273	.0369	.0464	.0558	.0652	.0744	.0836
9	.0088	.0127	.0175	.0224	.0272	.0320	.0367	.0415
10	.0039	.0059	.0083	.0107	.0132	.0156	.0180	.0204
11	.0017	.0027	.0039	.0051	.0063	.0076	.0088	.0100
12	.0007	.0012	.0018	.0024	.0031	.0037	.0043	.0049
13	.0003	.0005	.0009	.0012	.0015	.0018	.0020	.0024
14	.0001	.0002	.0004	.0005	.0007	.0009	.0010	.0012
15	.0000	.0001	.0002	.0003	.0003	.0004	.0005	.0006
				p=1/3				
4	.1084	.1384	.1745	.2090	.2421	.2739		
5	.0341	.0448	.0580	.0711	.0839	.0966	.1091	.1214
6	.0105	.0141	.0187	.0232	.0277	.0321	.0366	.0410
7	.0032	.0044	.0059	.0075	.0090	.0105	.0120	.0135
8	.0010	.0014	.0019	.0024	.0029	.0034	.0039	.0044
9	.0003	.0004	.0006	.0008	.0009	.0011	.0013	.0014
10	.0001	.0001	.0002	.0002	.0003	.0004	.0004	.0005
				p=1/4				
3	.1601	.2002	.2476	.2922	.3342			
4	.0387	.0501	.0641	.0779	.0915	.1048	.1180	.1310
5	.0090	.0119	.0156	.0192	.0228	.0264	.0299	.0335
6	.0021	.0028	.0037	.0046	.0055	.0065	.0074	.0083
7	.0005	.0007	.0009	.0011	.0013	.0016	.0018	.0020
8	.0001	.0002	.0002	.0003	.0003	.0004	.0004	.0005
				p=1/5				
2	.4107							
3	.0889	.1124	.1410	.1687	.1955	.2214	.2465	.2712
4	.0169	.0219	.0282	.0345	.0407	.0468	.0529	.0590
5	.0031	.0042	.0054	.0067	.0080	.0092	.0105	.0118
6	.0006	.0008	.0010	.0013	.0015	.0018	.0021	.0023
7	.0001	.0001	.0002	.0002	.0003	.0004	.0004	.0005

The first feature mentioned above becomes important when it is desirable to minimize over-learning in experiments in which the subject is likely to show sudden "insightful" learning or problem-solution. In

TABLE II

THE LARGEST NUMBER OF TRIALS FOR WHICH THE PROBABILITY OF A RUN OF AT LEAST S SUCCESSES IS LESS THAN ε WHEN THE PROBABILITY OF A SUCCESS ON A SINGLE TRIAL IS p.

p=1/2					
S \ ε	.001	.005	.010	.050	.100
4	—	—	—	—	5
5	—	—	—	6	9
6	—	—	—	10	16
7	—	—	7	17	31
8	—	8	11	31	59
9	—	12	17	59	113
10	10	18	28	112	222
11	13	29	50	218	439
12	18	51	92	429	871
13	27	93	175	850	1735
14	44	176	341	1692	3463
15	78	341	671	3374	6917

p=1/3					
S \ ε	.001	.005	.010	.050	.100
4	—	—	—	8	14
5	—	5	7	21	41
6	—	9	15	60	119
7	8	21	38	173	350
8	16	55	105	510	1042
9	37	155	304	1521	3117
10	97	452	898	4551	9340

p=1/4					
S \ ε	.001	.005	.010	.050	.100
3	—	—	—	5	10
4	—	4	6	19	38
5	5	10	17	73	146
6	10	32	59	284	579
7	27	115	225	1125	2306
8	94	444	884	4486	9208

p=1/5					
S \ ε	.001	.005	.010	.050	.100
2	—	—	—	2	3
3	—	—	3	9	17
4	—	6	10	42	84
5	7	23	42	203	414
6	24	102	200	1006	2061
7	103	495	987	5014	10294

such cases, a reduction of the probability of a chance success on a single trial[3] will reduce the length of the run necessary to indicate learning. Short runs might also be of value in detecting "hypothesis" behavior and other theoretically consistent but evanescent forms of behavior.

[3] Reduction of the a priori probability of a chance success may be accomplished by adding to the number of possible but incorrect responses.

The second feature mentioned above leads to a difficulty when training is to be given until the subject's performance meets any constant criterion. Suppose, for example, an experimenter is running 25 trials a day on a simple discrimination problem in which the subject has to pick the correct *one* of *two* objects. He notes that $p=\frac{1}{2}$, and from Table I, that a run of *eight* or more correct responses ($P=.0369$) would be significant at the 5% level and decides to use this as his criterion of learning. Suppose that his subject fails to achieve this criterion the first day. The experimenter might then decide to continue training until, on some day, the criterion is met. With sufficient persistence on the part of the experimenter, the probability of achieving this criterion will approach 1.00 even though the subject always responds randomly with respect to the critical cue, throughout the experiment. Correct use of the runs criteria requires that all trials within a previously selected limited series be taken into account. This condition may be met in any of a number of ways. For example, the experimenter may decide *in advance* to limit his experiment to a given number of trials, say, 500. Or, again, he may decide *in advance* that the 25 trials shall be *the* one test series. Some limit must be set—in other words, ***failure of the experiment as well as its success must be made possible.***

To make convenient the application of the runs criteria when a large total number of trails is contemplated, Table II is presented in which the maximum series length is given such that a run of at least S correct responses is significant at the given confidence level, ϵ.

In using Table II, the experimenter described above would presumably decide, in advance, that if the subject has not achieved the criterion in a certain number of trials, say, 429, the experiment will be terminated. If 429 were selected, the significant ($\epsilon<.05$) run would be 12 correct responses. Here the daily records would be pooled so that a run of correct responses might extend over two days or two 25-trial series. Again it will be noted, that had the single trial probability of a chance success been smaller than $\frac{1}{2}$, the significant run would have been quite short.

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ENCYCLOPEDIA OF PSYCHOLOGY

A SPECIAL REVIEW*

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This volume is an attempt to present the field of psychology in encyclopedic form. It consists of approximately 100 articles representing 82 contributors. Some of the articles discuss such broad subjects as *Child Psychology, Emotion, Personality*, or *Motivation*. Others—for example, *Aspects of Blindness, Psychological Psychodrama* and *National Council of Women Psychologists*—are confined to relatively narrow and specialized topics. In general, emphasis is placed upon contemporary problems, approaches, and findings, although an occasional article presents a somewhat exhaustive historical account. Some contributors attempt to give a thorough review of their topics with carefully annotated references to the literature. Many writers content themselves with general discussions in which their own interests and theories take precedence over the work of others. From the viewpoint of both subject matter and style, therefore, there is an inexcusable lack of uniformity or consistency.

In a preface, the editor writes that one purpose of this volume was "... to emphasize some of the trends in contemporary psychology which seem to have supplanted much of the traditional material." This appears to indicate that some general plan was followed in determining the apportionment of space to the various subjects. Unfortunately, such a plan is not readily discernible. Certainly one hesitates to believe that the allotment of more space to *Parapsychology* and *Pseudopsychology* than to *Learning, Experimental Psychology, Industrial Psychology* or *Aptitudes and Aptitude Testing* is indicative of the Editor's opinions about current trends in the field. While some plan may be discovered in the fact that topics which relate to abnormal psychology and psychiatry are assigned over 200 pages of the 897 total, one must wonder if it were really planned that *Psychosomatics* should receive 43 pages, *Psychotherapy*, 16, and *Religion and Psychiatry*, 17. Similarly it is difficult to grasp a plan which leads to the inclusion of separate sections on *Frames of Reference in Psychology, Phenomenology, Philosophical Psychology, Points of View, Scientific Law in Psychology*, and *Systematic Psychology*.

* HARRIMAN, PHILIP L. (ED.) *Encyclopedia of psychology*. New York: The Philosophical Library, 1946. Pp. vii+897.

Equally difficult is the explanation of a 13-page section on *Language and Psychology*, a 9-page section on *Language, Psychology of*, and a 33-page section on *Semantics, Language*. When these assignments are compared to the 11 pages given to *Learning*, the 4 pages given to *Industrial Psychology*, and the 16 pages given to *Personnel, Teacher*, the existence of any plan at all is placed in doubt.

This doubt is not dissipated by a closer examination of the book. Instead, as in "Alice in Wonderland," things get "curiouser and curiouser." It is a disturbing discovery that 8 pages are devoted to a discussion of the *Eye*, including some material which is largely duplicated in a section on *Color*, but that only 4 pages are given to an anatomical description of the ear, allowing a few paragraphs in the section on *Receptors and Effectors* for the facts of audition. Does this mean that current trends in psychology have forced hearing out of style? Is there significance in the fact that Jung is the only psychologist to appear in a section under his own name or that he, Adler, Porteus, and Rorschach are the only individuals referred to in the index?

Elsewhere there is further evidence that the editor failed to plan or, indeed, to edit. According to the Preface, the volume was intended " . . . to meet the requirements of the serious investigator who wishes to acquaint himself with various topics in modern psychology which lie outside his field of special interest and competence." It was also supposed to be serviceable "in the enrichment of various topics by initiating the student into an intensive exploration of the primary data." How will the serious investigator react when he finds that some articles are accompanied by complete bibliographies while others have none? It is to be hoped that he will not suppose that bibliographies are omitted only for those topics for which publications are lacking. Certainly, it would be unfortunate if a student arrived at that conclusion when he discovered his intensive exploration of the primary data in the field of learning blocked by the absence of a bibliography while one of 37 titles beckoned him into the field of *Porteus Maze Tests*.

Other evidence of inadequate editing is found in the fact that the bibliographies are presented in inconsistent formats which appear to depend upon the habits and vagaries of the individual contributors. This defect is only one of a number of symptoms. For example, the section on *Genetic Psychology, Experimental*, refers by name and number to over 180 sources but the editor somehow failed to append the bibliography. These faults do not increase the reader's confidence. They do increase the sympathy he feels for the contributor who, in her article on *Color*, blithely wrote "See heredity," and who now knows that there

is no section on heredity. The "index" of this encyclopedia is a 3-page table of contents with an attempt at cross-indexing which is exemplified by the entry, "Ear—see Eye; Receptors and Effectors." Incidentally, the "index" includes no page numbers.

Some may feel that these criticisms are of a petty variety. They are presented, however, to support a grave charge. In brief, it is believed that the contents of this volume were amassed by the process of selecting certain contributors, and in general, of allowing their own interests, freedom of time, notions of style, etc., to determine what, how, and how much they wrote. It is submitted that even if contributors are maximally expert in their particular fields, a useful reference volume cannot be produced unless someone assumes responsibility for over-all planning and presentation. In this case we may question not only the discharge of this responsibility but also the assertion of the Editor that these articles are written "by those who have achieved distinction through contributions to special areas within the science of psychology." Many of the contributors are indeed eminent in the field of which they write. But many others are relatively unknown. Whether the writers deserve to be unknown is not to be discussed here. We may, however, leave it to the reader to reconcile the editor's statement with the fact that the section on *Emotion*, *Frames of Reference in Psychology*, *Jung*, *Personal Document Analysis*, and *Phenomenology* are the work of a single contributor.

Admittedly, this is not a pleasant review. It is not intended to be. The title of this book is such as to place it almost inevitably in every college library and in the hands of many laymen who honestly desire to extend their knowledge of the field. At its best it will give students and laymen alike a mistaken notion about the nature of psychology as a science. At its worst, it will make them think they know more than they do or will make the more perspicacious of them think psychologists agree even less than they do. The thinking student who reads the section on *Conditioning* and is told that " . . . Hull has formulated by far the most extensive and consistent theory of learning thus far offered . . . " is going to perceive that something is radically wrong when he reads the section on *Learning* and finds that Hull's work is not mentioned. Something is surely wrong; but with the book, not with the field. Should not something be done to protect psychology from this sort of thing?

THE RELATION OF EMOTIONAL ADJUSTMENT TO INTELLECTUAL FUNCTION—A NOTE

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It has often been asserted that changes in a child's emotional adjustment are reflected in corresponding changes in his tested intelligence. Most clinical psychologists have had occasion to report that a measured IQ has seemed too low an estimate of a disturbed or anxious child's "real" intelligence. This reservation is usually based on observations of anxiety and non-intellectual attitudes which operate to decrease efficiency in the test situation. The statement implies that if these factors were not present the measured IQ would be higher. Since tests of intellectual ability are usually standardized on non-clinic populations in whom these factors are minimal, it is sometimes necessary to make such qualifications.

However, it is commonly asserted that there is a more pervasive kind of impairment of intellectual function which operates to lower efficiency, not only in the test situation but in other situations demanding intellectual effort as well. It is further asserted that changes in the life situation operating over the course of time, or expedited by psychotherapy or other manipulation, so modify these inhibiting factors that subsequent IQ's are significantly different from the initial measure. Demonstration of the existence of such factors requires, of course, measurement over a period of time of changes which are greater than might be expected from the inadequacies of the test and the difficulties inherent in testing emotionally disturbed children.

The most recent study purporting to demonstrate such changes is that of Despert and Pierce (1). Their purpose was to "ascertain whether any relation can be established between emotional adjustment as shown in the total record and intellectual function as determined through psychometric testing" (p. 7). The revised Stanford-Binet was administered to 39 children before and after attendance at nursery school. Available also was information from which they made evaluations of personality, of socio-emotional adjustment and of the family situation. After the administration of the final test, the records were examined for factors which might account for observed increases or decreases in IQ. According to the authors: "The results of the psychometric tests and the findings related to socio-emotional adjustment were analyzed *inde-*

pendently" (italics theirs). However, they do not use the word "independently" in its usual sense; apparently they did not attempt to predict the measured intellectual changes from personality data alone. Rather, the procedure seems to have been to examine the test-retest differences, to select those cases showing greatest change and then to examine the other data for possible reasons. In every case showing decline it was possible to find some plausible reason such as parental discord, the birth of a sib, a hostile nurse, etc. The factors associated with rises were more obscure, the phrase, "improved emotional and social adjustment," appearing several times. The authors do not hesitate to ascribe causal effect to these factors.

However, examination and recalculation of their data indicate that the changes can be explained by a simpler hypothesis. It is possible, knowing only the initial IQ's and the standardization data of the test, to predict for the group as a whole the mean IQ and its variability on a second test. It is possible also to predict for any given IQ range within the group both the direction and the magnitude of change on a second test. These predictions agree almost perfectly with the results presented by Despert and Pierce. For example, there are two cases with IQ's in the range above 150; one child tested at 155, another at 157. On statistical grounds alone (see below), one would predict that on a retest (an alternative form administered the same day) initial IQ's of 155 would decline to 142, and initial IQ's of 157 to 143. The actual findings are 141 in the first case and 144 in the second. These two are among the cases for whom the authors produce data accumulated over a period of time to account for a "significant" change (10 points or more). Also on statistical grounds, one is not surprised to find that those cases showing significant declines averaged 34 points higher initially than a group showing comparable rises.

The initial mean IQ of the ten cases selected by Despert and Pierce as showing a significant change downward is 140 and that of the group changing upward 106. The uncorrected correlation ratio between initial score and loss is .80 ($N=39$, $P<.001$). Apparently selecting for direction and size of change has isolated groups initially quite different from each other. However, on the second test, these groups approach each other very closely; the group showing losses averaged 127 and the group showing gains, 121. Thus both groups tend to approximate the mean of all cases. A conclusion one might draw is that if a child scores high initially, then bad things are going to happen to him, and if he scores low, good things are going to happen so that he ends up closer to the mean of the group in either case. A more credible explanation is in term of statistical regression.

Statistical regression is the name for the phenomenon that a group of scores selected because of their deviation from the mean of a population will, on a second test, more closely approximate the population mean. A score on a test may be thought of as having two components: the "true" amount of the trait measured plus a variable error. If from a group of test scores, one selects the highest, the effect is to increase the probability of selecting those which have associated with the true score the greatest amount of plus error. On a second test, the errors would be just as likely to be in the opposite direction and consequently the second set of scores would be lower. The converse would be true for scores below the mean. This principle is nicely illustrated by the Despert-Pierce data. It is possible to predict precisely the amount by which any selected group of scores will regress toward the mean. Since the amount of regression varies inversely with the reliability of the test, it is necessary to know the reliability coefficient.[1] The only other data needed are the initial mean and standard deviation for the total group, and, if they change significantly, these same values for the second administration.

In the Despert-Pierce data the means from the two administrations were within one point of each other. The standard deviations varied by the amount and in the direction to be expected for children of this age (**3**). Predictions of regressed mean values were made for various IQ levels from the usual regression equations. If the total group is divided at the mean, those scoring above the mean averaged 135 on the first test. The predicted mean for the second test was 130, exactly the same as the obtained figure. For the cases scoring below the mean the average score was 105 on the original test and 114 on the second test. The predicted mean was 112. This is an insignificant difference (*P* equals 0.80 for 17 d. f.). Breaking down the groups more finely gives the data shown in Table I.

Examination of this table makes it clear that those levels farthest from the mean regressed the most. With this small number of cases, it is impressive how closely the predicted values approach the observed. Obviously no hypothesis other than regression is necessary to account for the changes. To isolate particular individuals from these groups as

[1] The test-retest correlation (rho) calculated from the Despert-Pierce data is .69. Because of the time interval between the tests and the (probably) different functions measured at the two age levels, this figure cannot be accepted as a measure of reliability. In the calculations to follow we have used the reliability coefficients appropriate to age and IQ levels as reported by McNemar (**3**). When we made the same calculations using the value .69, the observed and predicted values were even more similar than those reported in the text.

showing spectacular changes is to select cases where the errors have been in opposite directions on the two administrations, and to assign psychological significance to statistical artifact.

We have labored this criticism so far because of the importance of the problem which Despert and Pierce purported to study. That what are called socio-emotional factors interfere with (or facilitate) intellectual achievement there can be no doubt. As an example one might cite the nine percent of men in the Stanford Gifted Group who as young

TABLE I

COMPARISON OF ACTUAL RETEST MEANS AND THOSE PREDICTED FROM REGRESSION EQUATIONS

*IQ Range**	*N*	*Mean of first test*	*Mean of second test*		*Dis-crepancy*
			Predicted	*Observed*	
67–103	7	93.0	104.4	106.3	+1.9
104–121	11	113.0	117.2	118.9	+1.7
122–139	15	129.7	127.4	125.8	−1.6
140–157	6	147.7	137.8	139.2	+1.4

* Class intervals based on size of standard deviation.

adults failed to rise above the level of semi-skilled labor (4). Similarly every one knows examples of superior achievement in individuals of mediocre ability. But that the IQ itself is subject to socio-emotional factors is not obvious nor, as far as we know, has it been demonstrated. Proof would require an experimental design less contaminated and statistically more sophisticated than the study discussed here. Our recalculation of the data offers strong presumptive evidence that the influence of such factors is minimal.

There remains the possibility that more severe forms of maladjustment than are represented in a nursery school population may depress measured IQ's and that more intensive therapy may raise them. Published data on this point are equivocal at best (2). Our own data, based on 30 clinic children, are too unwieldy for a simple quantitative summary. Study of individual cases, taking account of age and IQ level (factors related to the size of the standard error), special handicaps, conditions of testing, etc. has led to the tentative generalization that within wide ranges of different kinds of personality and behavior problems,[2] and within a wide range of "cooperation" in the test situation, a

[2] We do not mean to include here psychotic conditions, or organic brain disease or other physical conditions known to affect cortical function.

measured IQ remains fairly stable, even after intensive therapy and improvement in adjustment. The fact that changes in IQ have been so small has often been disturbing to therapists working with the patients. A number of factors are at work here. First there is the fact that in our culture a high IQ is a "good thing." Second, a therapist has some personal stake in the child and his improvement—by identification with the child, as a validation of his therapeutic skill, etc. Third, because there are no unambiguous indices of improvement in adjustment, the IQ is frequently seized upon, erroneously, of course, as such a measure. This is an unfortunate state of affairs, particularly because not only psychotherapists, but also educators, workers in social agencies, and others are motivated to look eagerly for rises in IQ.

A further implication of the principle of regression to clinical work should be mentioned. Schools for mental defectives and other agencies concerned with children of below average intelligence often have the experience of obtaining higher IQ's on re-testing than those reported by a referring agency. This is a consequence of the fact that a low IQ is one of the factors for which children are referred to such agencies. If the principle of regression is not understood, there are at least two possible misinterpretations of the higher IQ's on re-test. If the second test is given shortly after the first, doubt may be cast on the adequacy of the first testing. If the second test is delayed until after a period of training or therapy, the conclusion may be drawn that the program has effected the rise in IQ. As in the case of the Despert-Pierce data, the effect of regression is to move the scores closer to the mean of the group from which they were selected, in this case, the mean of the general population.

It is easy to understand the motivation of those who would like to raise IQ's. It should be obvious, however, that improved socio-emotional adjustment is an end in itself, and any program effecting it need not depend on changes in measured intelligence for confirmation of its success.

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CORRIGENDA

In re-writing and re-copying the material for my article entitled "The Detection and Treatment of Accident-Prone Drivers," which appeared in the *Psychological Bulletin* in November, 1946 (vol. 43, pp. 489–532) I overlooked several small errors which may annoy the reader unless he correct them. They are as follows:

p. 492, 2d line from bottom of text:
  For −0.21 put −0.021
p. 492, second footnote:
  For −0.27 put −0.027
  For −0.26 put −0.026
  For 0.21 put 0.021
  For "counter" put "according."
p. 493, line 3:
  For 0.37 put 0.037
  For 0.18 put 0.018
p. 494, last line:
  For 0.84 put 0.79
p. 500, line 2:
  For 19 put 10
p. 510, par. 1, line 3:
  For r_{12} put r^2_{12}

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BOOK REVIEWS

Dunlap, Knight. *Religion: its functions in human life.* New York: McGraw-Hill, 1946. Pp. xi+362.

This book is "a study of religion from the viewpoint of psychology," an attempt to bring together in systematic fashion the characteristic phenomena of religious behavior and on the basis of these data to formulate a theory of religion which explains them. The theory thus arrived at is stated as follows:

> . . . in the early stages of religious development religion can be described as the attempt to do what man does not know how to do and to know the unknowable, . . . in its present stage . . . religion is the institution or feature of culture, which undertakes, in the service of mankind, the functions for which no other institution is as yet adequately prepared (p. 321).

The book is basically a volume in comparative religion. Its plan consists in a series of religious topics, under each of which is presented typical comparative religious data followed by observations concerning the psychological significance of these data. These topics include: religion as morality, concepts of divinities, the concept of sin, the concepts of the other world, the evolution of divinities, the role of desire in religion, religious symbolism, conversion, the nature of religion, and the future of religion in civilization, to mention only a few.

The author makes generalizations repeatedly in the comparative religion portions of the book which can be made only by authorities in the field. In the psychological sections of the book, several hundred (estimated) generalizations are made, of which his over-all description of religion, previously quoted, is the central one. Many of these generalizations seem to be justified by the data on which they are based, assuming the completeness of those data. For example there are many such well substantiated observations as these: "Gods are not essential for religion . . . All religions, however, . . . reach a stage in which gods and goddesses appear and multiply (p. 98). . . only the concept of a personal god can satisfy man's feeling toward divinity. . . The substitution of an impersonal world power for a personal god is a regression toward a more primitive conception" . . . (p. 336). Many of these conclusions however would seem to be easily replaced by other and often contradictory hypotheses to explain the same data.

There is nothing in the volume that remotely touches on experimental psychology. The major concepts in the psychology of personality are only occasionally mentioned and then not systematically. Many psychologists will be astonished at this statement: "We make an unjustified assumption even if we ascribe motivation to animals, for motivation involves desire, which is a complex process probably above

the mental level even of the ape" (p. 126). This is especially interesting because it is followed by a list of primary desires including the desire for food and drink, rest, activity, and the erotic desire, which do not seem to be absent from most animals.

Three religious concepts are dealt with in generalizations which appear to be less profound than the problems themselves deserve. They are the concept of salvation, the problem of evil, and the problem of immortality. Two quotations are illustrative. " 'Salvation' may be loosely defined as the process, or the condition, of being saved; that is, of being rescued from, or protected from, some harm, danger, risk or hazard" (p. 82). This is purely negative. In explaining evil, Dunlap says: "God, creating man in His image, made him a person and thus permitted man to create evil. This may not be a satisfactory solution . . . but it is the best any theologian has been able to think out" (p. 254).

Those psychologists who seem to believe that an adequate interpretation of human personality and social behavior can be made without a serious evaluation of religious behavior ought to read this volume. However little or much one may agree with the author, he could not fail to be convinced of the importance of the field.

Those whose religious convictions make it difficult for them to study it with truly scientific objectivity will do well to examine this largely dispassionate discussion of religious ideas. It will show them other points of view than their own.

Despite this somewhat critical review, the reviewer finds himself in awe of and with considerable admiration for the courage of the author in undertaking so gigantic a task. The book will probably not receive nearly the breadth of reading it deserves. As a source book in religion and social psychology it must be given an important place in every library in which students investigate related problems.

ERNEST M. LIGON.

Union College.

FRIEDMAN, BERTHA B. *Foundations of the measurement of values: The methodology of location and quantification.* New York: Teachers College, Columbia University, 1946. Pp. viii+227.

Critical of the majority of present attitude and interest tests because "little attention was paid to the theories upon which the tests rested, or to the means used to validate the test," the author attempts to erect "a framework upon which value-testing instruments could be built."

For this purpose she examines, at times in exhaustive detail, a number of questions directly or indirectly related to the measurement of values. Among these are the definition, location, and quantification

of values, and types of scales which have been, or might be used in the measurement of values.

Since the purpose of value-measuring instruments is to aid in the prediction of conduct, values are defined in terms of behavior. As indices of value, it is proposed to use the way in which a person spends his time, money, or energy. Data may be obtained through overt behavior or through verbal statements. Each method has its inadequacies. There is a further problem in the naming and classification of values, but here, as elsewhere in measurement, decisions should take account of the purpose of the investigator.

Quantification of values may be secured through measuring the amount of time or money expended or the number of instances where *X* is chosen in preference to *Y*. However, the primary problem of quantification is the determination of the empirical meaning of scale scores and not the measurement of scale-point intervals. Psychological, and not merely logical, continua must be established.

Following the discussion of what constitutes measurement in the field of values—to which the greater portion of the book is devoted—the writer describes three experimental investigations of her own designed to contribute data on certain questions which she has previously raised.

The book contains a comprehensive analysis of many problems basic to the measurement of values. Pitfalls and difficulties of great variety are pointed out. In fact, the writer is led at one point to inquire, "Is the possibility of locating values, then, hopeless?" In reply she states, "But this inquiry, it must be remembered, did not begin with any assumption that values could always be accurately located, let alone accurately quantified. Instead, it set as its goal the improvement of the location and quantification of values, which it recognized was an activity that was being pursued almost wholly without concern for methods which would bring increasingly better results."

To many readers the chief interest of the book will lie in the specific questions it raises in regard to the Thurstone scales and other widely used scales for the measurement of attitudes or values.

While the author has undoubtedly made a contribution to the field of value-measurement, the discussion at times seems unnecessarily circuitous, and the reader is led to wonder whether the longest way round is really the shortest way home. And it must be added in all sadness that the exhaustive consideration of theory does not appear to the reviewer to have improved the "practice" in values measurement as represented in the techniques used by the author in her own experimental investigations.

ELIZABETH DUFFY.

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MAYS, A. B. *The concept of vocational education in the thinking of the general educator, 1845 to 1945.* University of Illinois Bulletin, 1946. Pp. 107.

This development of general educators' ideas provides background for understanding educational practices. It has wider significance than an historical account of slow progress from cultural to balanced-general-vocational philosophies of education. One may infer that during the last century two opposing forces controlled ideas: the traditional, advocating a cultural education for higher conformities to the regional culture; the newer, advocating vocational education for higher efficiencies to serve the needs of agriculture, commerce and industry. The general educators' ideas guarded the cultural features with few exceptions. "Freeing" ideas came from insights related to widespread survival needs and satisfaction drives of economic interests and local communities—a few becoming social movements, chiefly with Federal support.

Maladaptive social implications such as support for hardening unequal caste distinctions are seen for the cultural trend. Such insights are lacking for the economic pressures and competitive struggles and techniques, though they are implicit in symptoms of tensions and disturbances of thinking.

The author faces the future with optimism on the basis of the balanced solution, though this looks like an unstable affair. Progress for the atomic age should develop the abandonment of competitive struggles and wider educational diets than balancing political and economic interests alone. The author and his selected quotations neglect needs related to building a world culture to secure cooperation and security; and to implementing the drives of free individuals and groups to realize their personal, social, and creative interests and develop their distinctive talents.

JAMES L. GRAHAM.

Lehigh University.

KINGSLEY, HOWARD L. *The nature and conditions of learning.* New York: Prentice-Hall, 1946. Pp. xvi+579.

When one attempts to evaluate a new textbook two points of view compete for his attention as frames of reference. The first appraises the newcomer with reference to earlier texts and the trends of development which they exhibited. The second derives from experience with the needs which such a text must meet. Accordingly, on the one hand, the reviewer looks for changes in scope and emphasis in comparison with previous works in the field. Once these have been identified, he evaluates the salient points of the new work with respect to degree of approximation to the structure of what the reviewer regards as a desirable textbook.

Professor Kingsley's text, when viewed within these frames of reference, impresses the reviewer favorably on both counts. More than

any other text with which he is familiar, the book presents psychological material in a manner most likely to assist the student, preparing for an educational career, in acquiring the elementary background of principle and technique prerequisite to advanced study. According to the author, the treatment of the field of educational subjects:

> . . . rests on the conviction that learning is the core of educational psychology, that laboratory research provides the most reliable data concerning the fundamental nature and conditions of learning, and that the educational psychology of learning must be based solidly on the fruits of the laboratory. . . . An effort has been made to secure a balance of experimental evidence, illustration, interpretation, summary, and application. Illustrations are provided from everyday life and from the classroom to promote the comprehension of principles and to help bridge the gap between the research laboratory and the classroom.

These quotations from the preface are amply substantiated in the body of the text. Part I deals with the development of the subject matter and techniques of educational psychology and their role in the educative process, as well as treating comprehensively the fundamental nature of learning. Part II treats the topics of maturation, varied activity, repetition, motivation, conditioning, and relationship and organization as six fundamental principles of learning. Part III deals with learning as the improvement of performance and with fatigue and physical handicaps as factors detrimental to learning. Part IV deals with seven forms of learning treated under the heads of the development of motor skills, the development of perception, memorizing, the development of comprehension, thinking as problem-solving activity, the development of emotional activity, and the development of attitudes and ideals. Part V, the concluding section, treats of the retention and transfer of learning.

The book as a whole hews closer to the traditional pattern of texts in educational psychology than certain of the more recent books in this field. In many ways it just misses being an exceptionally well-planned text of the traditional sort. Thus it may be expected to be accorded a mingled reception. Consideration of the psychological aspects of educational development as it relates to the social development of the child and adolescent in our culture tends to be slighted. Many instructors in educational psychology, however, will be just as well content. Such matters, it will be maintained, should be left to texts in genetic and child psychology. But since state education department are notoriously loathe to give certification credit for courses lacking the terms education or educational in their titles, the prospective teacher is all too often denied opportunity of instruction in this important aspect of individual development.

Well-balanced chapters on the development of emotional activity and of attitudes and ideals do much to overcome this deficiency, but in the reviewer's opinion they are not completely adequate. Nevertheless, the book is a scholarly, readable, and current presentation of the sub-

ject which should prove to be a desirable text. The serious student will welcome the comprehensive bibliographies which accompany each chapter.

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TRAXLER, ARTHUR E., & TOWNSEND, AGATHA. *Another five years of research in reading*. Educational Records Bulletin No. 46. New York: Educational Records Bureau, 1946. Pp. 192.

Of all the research areas represented by the subjects of the school curriculum, that of reading has been, and still is, the most intensively cultivated. During the five-year period 1940-1945 — and these were "war years" — an average of more than a hundred printed reports of investigations relating to reading appeared annually. In view of this prolific output of research it is important now and then to summarize and to take stock. This the present monograph endeavors to do for the period just noted. Taken together with *Ten Years of Research in Reading* published under the same auspices in 1941, the monograph enables one to note trends with respect to problems studied and techniques employed.

Some of the major trends are recorded in a four-page "Introduction." In the following sixty-nine pages the research reviewed is classified and discussed under twenty topics. Pages 75-184 contain bibliographical data for 527 "selected" studies together with annotations limited to about one hundred words each. The monograph is concluded by an alphabetical list of the authors of the reading reports and by a subject index.

The contents of the monograph are perhaps best disclosed by the list of topics or problems around which research centered in the period 1940-1945:

1. Reading readiness and beginning reading
2. Reading interests
3. Reading in connection with the school subjects
4. The vocabulary and content of elementary-school readers
5. Vocabulary lists and vocabulary building
6. Phonics
7. Reading tests and testing procedures
8. Speed of reading
9. Eye-movements and reading ability
10. Visual, auditory, and speech defects
11. Dominance, handedness, eyedness and reversals
12. Relationship between reading and other factors
13. Activity programs and reading achievement
14. Hygiene of reading
15. Diagnosis of reading difficulty
16. Remedial and corrective teaching of reading
17. Developmental reading

18. Various aspects of reading (e.g., adult reading)
19. Reading bibliographies and summaries
20. Books and monographs on reading and the teaching of reading.

During the past fifteen years the authors note increased research interest in the problems numbered above as (1), (2), (3), (4), (5), (6), (8), (14), and (17), with some loss of interest in problems (11) and (15) especially.

The authors of the monograph are entirely competent as students of reading, and their comments and criticisms are insightful and suggestive. Consequently, their summary is obviously of signal importance to educators, whether they be concerned with the practical business of teaching children to read in the classroom or with the production of original research. But the monograph should be of value to psychologists as well, for many of the problems discussed are fully as significant psychologically as they are educationally.

William A. Brownell.

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Moncrieff, R. W. *The chemical senses.* New York: John Wiley, 1946. Pp. vii+424.

First published in Great Britain in 1944, this book, written by an associate of the Royal Institute of Chemists, covers a wide range of subject matter on taste and smell, taken from the fields of biology, chemistry, electrophysiology, physics, and psychology. Although the author has done an admirable job of coordinating the technical languages of these sciences into a very readable book, one might well ask just what audience or audiences the book was written for. There is no preface disclosing the author's intent.

Of the fourteen chapters the first is devoted to the place of the chemical senses in the animal and human world. Here the author gives a review of the importance of the chemical senses in organisms, ranging from a description of the avoidance of poisons by lower animals to the use of odors in medical diagnosis. The material of four chapters dealing with taste and smell receptors, the nervous system, olfaction, and gustation is very similiar to that found in a general psychology text. Chapter 6, which is entitled "The Common Chemical Sense" might well confuse the reader who is unsophisticated in the traditional problems of sense classification. In describing this "separate sense" (p. 130) the author states: "It is clear that the chemical sense is distinct from smell, taste, touch, and pain, and has a separate set of receptors. These receptors are the free nerve endings of spinal and cranial nerves, and are comparable in distinctiveness with heat, cold, or pain receptors, but are more localized." There are various ways of classifying the senses and one of them is based on the stimuli adequate for them. However, this mode of classification has never had the appeal of others. We could speak of the "electrical sense" or the "mechanical sense" or even

of the "sense of noise," but we do not and for good reasons. It would seem better to regard the pain aroused by chemicals as belonging to the same "sense" as that elicited by mechanical, thermal, and electrical stimuli. This chapter describes the common chemical irritants which arouse various sensations, from lachrymators to vesicants. One chapter is devoted to chemical sensibility in lower animals.

The author devotes a chapter to the classification of odors, pointing out the values and limitations of such systems as have been proposed by Rimmel, Zwaardemaker, and Henning. Three chapters on chemical constitution and the physical properties of odorous and taste materials provide an excellent account of the descriptions of chemicals as related to the various sensory experiences they can arouse. In summarizing these relationships into one hundred and twenty "general principles" (e.g., the main factor in determining odour is the architectural type of the molecule) the author states that "they are open to criticism, but the subject is in an early stage of development, and if they are criticized and replaced by a better lot they will have served their purpose." Chapter 12 includes a critical analysis of the theories of odor, with emphasis upon the constitution of odorous particles and the way in which they come into contact with the olfactory apparatus. The salient features of the theories of twenty-three authors are contained in a summary table for purposes of comparison. The author states that ". . . the transmission of the stimulus from the receptor to the brain is established fact" and adds that the nature of the brain processes is still a mystery. Little is said about theories on the perceptual aspects of odor.

Perfumes and essences, flavor and food, are discussed in the remaining two chapters. Methods are described for separating essential oils for use in perfumes and essences. The chief components of some seventy oils are given in tabular form, showing their various odorous qualities. A detailed chemical analysis of some of the more common oils is given, together with a description of their commercial use. Flavor is regarded as a complex sensation, comprising taste, odor, and touch. Throughout, the author makes effective use of homely illustrations which add "flavor" to the text, such as adding glucose to wines, serving brandy from thin convex glasses, and masking castor oil with peppermint.

For a book which covers such a wide range of subject matter, written primarily around the chemistry of taste and smell as its core, *The Chemical Senses* is well integrated. It might well be placed on a reading list for some advanced courses in psychology. The book is also of value for psychologists as a reference source showing the relationships between chemical stimuli and chemoreception. References to original papers are given in the text, but no summary bibliography is included. The book contains a few charts and tables, numerous chemical formulae, a glossary of technical terms, and a good index.

B. von Haller Gilmer.

University of Virginia.